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AUSTRALASIAN MEDICAL CONGRESS.

(FORMERLY THE INTERCOLONIAL MEDICAL
CONGRESS OF AUSTRALASIA.)

TRANSACTIONS

OF

THE EIGHTH SESSION,

MELBOURNE, VICTORIA,

OCTOBER, 1908.

VOLUME I.

TRANSACTIONS.

CONJOINT SECTIONAL MEETINGS.

SECTION OF MEDICINE.

SECTION OF SURGERY.

VICTORIA :

J. KEMP, GOVERNMENT PRINTER, MELBOURNE.

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- Wolfenden, J. H., L.K.Q.C.P. Ire., L.R.C.S. Ire., Dunolly, Victoria.
- Wolfhagen, J. G., M.B., C.M. Edin., Hobart.
- Wood, A. Jeffreys, M.D., B.S. Melb., Collins-street, Melbourne.
- Wood, F. Aldersley, M.B., B.S. Melb., Springfield-avenue, Toorak, Victoria.
- Wood, P. M., M.R.C.S. Eng., L.R.C.P. Lond., Ashfield, Sydney.
- Wood, W. Atkinson, M.D., M.S. Melb., D.P.H., Toorak-road, South Yarra, Victoria.
- Worrall, Ralph, M.D. *et* Ch.M., Q.U.I., 183 Macquarie-street, Sydney.
- Young, E. H., M.B., Ch.B. Syd., Clifton, Queensland.
- Yuille, H. D., L.R.C.S. *et* P. Edin., Williamstown, Victoria.
- Yule, J. S., M.B., B.S. Melb., F.R.C.S. Eng., The Avenue, Royal Park, Melbourne.
- Zwar, B. T., M.D., M.S. Melb., 85 Collins-street, Melbourne.

HONORARY MEMBERS.

- P. Baracchi, Government Astronomer, and President of the Royal Society of Victoria.
 S. S. Cameron, Chief Government Veterinary Officer.
 L. Champion, President of the Pharmaceutical Society, Melbourne.
 C. Candler, late City Coroner, Melbourne Club, Melbourne.
 E. T. Drake, Victorian Statist, Record Office, Melbourne.
 Professor Ewart, University of Melbourne, Carlton, Victoria.
 Hon. Victor Nelson Hood, State Government House, Malvern, Victoria.
 John Iliffe, President of the Dental College, Melbourne.
 G. H. Knibbs, Commonwealth Government Statist.
 Professor Lyle, University of Melbourne, Carlton, Victoria.
 Professor Orme Masson, F.R.S., University of Melbourne, Carlton, Victoria.
 Geo. Morrison, late Under-Secretary, Victoria.
 Calder E. Oliver, Chief Engineer of the Melbourne and Metropolitan Board of Works, Melbourne.
 H. W. Shillinglaw, Secretary of the Pharmaceutical Society, Melbourne.
 Professor Baldwin Spencer, F.R.S., University of Melbourne, Carlton, Victoria.
 Thomas Tait, Chief Commissioner of Railways.
 F. Tate, I.S.O., Director of Education.
 Henry Gyles Turner, President of the Public Library, Melbourne.
 J. Prout-Webb, K.C., Master-in-Lunacy, Melbourne.

PROGRAMME OF PROCEEDINGS.

MONDAY, 12TH OCTOBER, TO SATURDAY, 17TH OCTOBER.

On Monday, 12th October, the Congress Office was opened in the Board Room of the Melbourne Hospital, which was placed at the disposal of the Executive by the Committee of Management. Programmes and invitations were distributed during the week. After Saturday, 17th October, the Congress Office was transferred to the University of Melbourne.

On Saturday afternoon, 17th October, Dr. and Mrs. Herman Lawrence, and Dr. and Mrs. Norman Macarthur, entertained a large number of Members of Congress and ladies at tea at the meeting of the Victoria Amateur Turf Club. The Committee of the Club forwarded a number of tickets of admission for the use of ladies accompanying Inter-State Members of Congress.

SUNDAY, 18TH OCTOBER.

In the morning, special services were held at many of the metropolitan churches.

At St. Paul's Cathedral, the service consisted of Choral Matins, the service being that of Martin in C, and the Anthem, Wesley's "Wilderness." The Rt. Rev. Dr. Mercer, Bishop of Tasmania, was the preacher, taking his text from the Epistle to the Philippians, iii., 21, "The body of our humiliation."

At St. Patrick's Cathedral, at the 11 o'clock Mass, the Bishop of Sandhurst (Dr. Reville) presiding at the throne, the Very Rev. James O'Dwyer, S.J., delivered an address on "The Catholic Church and Medical Science," taking the text of his sermon from Ecclesiasticus, 38, 1. "Honour the physician for the need thou hast of him; for the Most High hath created him. For all healing is from God."

At Scots Church, the Rev. Dr. Marshall was appointed by the administrative authorities of the Presbyterian Church to preach, and took as his subject "Reciprocities of Science and Religion," the text being from I. Corinthians, xii., 21, "The eye cannot say unto the hand, I have no need of thee."

On Sunday afternoon, the President of the Congress and Mrs. Allen were "At Home" at the University of Melbourne to the Chief Officers of Congress and the ladies accompanying them.

On the following days, the programme was as follows:—

MONDAY, 19TH OCTOBER.

9.30 a.m.—Registration of Members in Wilson Hall. Distribution of Programmes and Invitations.

10.30. a.m.—General Business Meeting in Wilson Hall.

2 p.m.—Demonstration of Surgical Cases at the Melbourne and Alfred Hospitals.

Inspection of new buildings of the Eye and Ear Hospital.

3.30 p.m.—Garden Party, given by the President of the Victorian Branch of the British Medical Association and Mrs. G. A. Syme, at "Blythswood," Kew, His Excellency the Governor of Victoria and Lady Gibson-Carmichael being present.

8 p.m.—Inaugural Meeting in Town Hall, Melbourne.

TUESDAY, 20TH OCTOBER.

9 a.m.—Demonstration in Museum of Pathology (Syphilis). Hospital Demonstrations, including—(1) Exhibition of Cases of Syphilitic Disease and Obstetrical Paralysis at the Children's Hospital; (2) Demonstration of Surgical Operations at St. Vincent's Hospital; (3) Demonstrations at the Women's Hospital.

10.15 a.m.—Address in Medicine by Dr. G. E. Rennie (Wilson Hall).

11 a.m.—Sectional Meetings (University).

1.15 p.m.—Lunch at University.

2 p.m.—Sectional Meetings (University).

3.30 p.m.—Garden Party, given by Mr. and Mrs. J. F. W. Payne, "Maritimo," South Yarra.

8 p.m.—Dinner at State Government House, given by His Excellency the Governor of Victoria to the Chief Officers of Congress.

Medical Students Theatre Night at the Princess' Theatre, under the patronage of the Congress.

WEDNESDAY, 21ST OCTOBER.

9 a.m.—Demonstration in Museum of Pathology (Medicine). Hospital Demonstrations, including demonstrations of deformities at the Children's Hospital.

9.30 a.m.—The Section of Neurology and Psychiatry met at the Receiving House, Royal Park.

10.15 a.m.—Discussion on Hospitals (Wilson Hall).

1.15 p.m.—Lunch at University.

2 p.m.—Address in Public Health and State Medicine by Dr. Mason (Wilson Hall).

3.30 p.m.—Garden Party, given by the Hon. Robert Harper, M.P., and Mrs. Harper, "Myoora," Toorak.

7.30 p.m.—Congress Dinner in the Town Hall, given by Victorian Members to Visiting Members, His Excellency the Governor of Victoria being present.

8.30 p.m.—"At Home," given to Ladies, by the Victorian Medical Women's Society, at the Paris Café.

THURSDAY, 22ND OCTOBER,

9 a.m.—Demonstration in Museum of Pathology (Surgery). Hospital Demonstrations, including—(1) Exhibition of Cases of Diseases of the Hip and Spine at the Children's Hospital; (2) Demonstration of Eye Cases and Spinal Analgesia at the St. Vincent's Hospital.

10.15 a.m.—Address in Surgery by Dr. Poulton (Wilson Hall).

11 a.m.—Sectional Meetings.

- 1.15 p.m.—Lunch at University.
 2 p.m.—Sectional Meetings (University).
 3.30 p.m.—“At Home,” for Ladies, given by Mrs. John Turnbull, at “Narambla,” Toorak.
 7.30 p.m.—Dinner, given by the Lieutenant-Governor and Lady Madden, at “Cloyne,” St. Kilda.
 9 p.m.—Ball, given by Victorian Officers of the Australian Army Medical Corps, at the Prahran Town Hall, His Excellency the Governor of Victoria and Lady Gibson-Carmichael being present.

FRIDAY, 23RD OCTOBER.

- 9 a.m.—Demonstration in Museum of Pathology (Gynæcology). Demonstrations of Bacteriological Specimens in the Bacteriological Laboratory. Hospital Demonstrations, including Cases of Infantile Paralysis at the Children's Hospital.
 10.15 a.m.—Sectional Meetings (University).
 10.30 a.m.—The Section of Neurology and Psychiatry met at the Kew Hospital for the Insane.
 2.15 p.m.—Discussion on Syphilis (Masonic Hall, Collins-street).
 4 p.m.—“At Home,” for Ladies, at the Alexandra (Ladies') Club.
 8 p.m.—Evening Party, given by the President of the Congress and Mrs. Allen, at the University, His Excellency the Governor of Victoria and Lady Gibson-Carmichael being present.

SATURDAY, 24TH OCTOBER.

- 10.15 a.m.—Final General Meeting (Masonic Hall, Collins-street).
 Afternoon and Evening.—“Henley on Yarra,” with a Congress House-boat, where the General Secretary of Congress and Mrs. H. C. Maudsley entertained in the afternoon, and a Committee of Victorian Members, organized by Dr. W. Atkinson Wood, entertained in the evening.

SUNDAY, 25TH OCTOBER.

In the afternoon, “At Homes” were given by Mr. and Mrs. J. A. Levey, “Bowden,” South Yarra; Dr. and Mrs. G. Rothwell Adam, “Carlton House,” East Melbourne; and Mr. and Mrs. J. P. Ryan, “Hybla,” Toorak.

The Hon. the Minister of Defence placed the Engineers' s.s. *Vulcan* at the disposal of Members, for an excursion round the Bay.

MONDAY, 26TH OCTOBER.

Visit of the Section of Neurology and Psychiatry to the Sunbury Hospital for the Insane.

8 p.m.—Evening Party at State Parliament House, given by the President of the Legislative Council and Lady Wrixon, His Excellency the Governor of Victoria and Lady Gibson-Carmichael being present.

COMBINED SECTIONAL MEETINGS.

Combined Sectional Meetings were held as follows:—

Tuesday, 20th October, 2 p.m.—Papers on Peritonitis, in the Sections of Surgery and of Obstetrics and Gynæcology. The President of the Section of Obstetrics and Gynæcology in the chair.

Thursday, 22nd October, 11 a.m.—Papers on Tubercle, in the Sections of Medicine and of Pathology and Bacteriology. The President of the Section of Medicine in the chair.

Thursday, 22nd October, 2 p.m.—Papers on Anæsthetics, in the Sections of Medicine and of Surgery. The President of the Section of Surgery in the chair.

DEMONSTRATIONS.

At the Alfred Hospital, Mr. R. Hamilton Russell showed—

- (a) Two cases of sub-periosteal bone implantation; in one, portion of the frontal bone had been removed; in the other a growth had been removed from the lower end of the ulna.
- (b) A case of absence of the radius, in which the lower end of the ulna had been split, and the lower end of the radial half moved outwards, to overcome the abduction of the hand.

Skiagrams were shown of the successful results of the two last operations.

- (c) Cases of tubercular joint disease.

Mr. O'Hara showed a case of appendicostomy.

At the Melbourne Hospital—

Dr. W. Moore showed a number of cases of nerve suture and plastic surgery on nerves.

Mr. G. A. Syme showed—

- (a) A series of cases of sarcoma and carcinoma, showing results of operation after many years, including sarcoma of brain and of tonsil, carcinoma of stomach, tongue, and breast.
- (b) Case of successful operation for intra-spinal hydatid, case of hydatid of ilium, and of hydatid of vertebra and rib.
- (c) Case of acute pancreatitis, successfully operated on four years before.
- (d) Case of trigeminal neuralgia, in which the Gasserian ganglion had been removed four years before.

At St. Vincent's Hospital—

Dr. Morton demonstrated Brophy's operation for cleft palate, illustrated by sections of a child's skull. A demonstration on anæsthesia was given on spinal analgesia, by Dr. B. T. Zwar;

regional local anæsthesia, Drs. J. T. Murphy and B. T. Zwar; and infiltration anæsthesia, with operation of thyroidectomy, Dr. T. P. Dunhill.

Dr. Herman Lawrence gave an exhibition of the methods of using liquid air and carbonic snow in the treatment of skin diseases.

The Medical Officers of the Queen Victoria Hospital entertained the visiting medical women Members of the Congress at the Hospital, on Monday morning, 19th October.

The Congress Museum was open daily, from 9 a.m. to 5 p.m. On Friday, 23rd October, Professor Berry conducted parties of Members through the Medical School and the Museum at 10 a.m., 11 a.m., and 12 noon.

THE CONGRESS MUSEUM AND EXHIBITION.

The Session of 1908 will be remembered for the excellent Exhibition and Museum, a monument to the active co-operation of members of Congress and the enterprise of various firms whose products interest medical men.

At previous Sessions of the Congress there had always been a small display, by permission of the executive, by a few of the more enterprising firms, in rooms or passage-ways adjacent to the meeting place of the Congress. At the Adelaide Session there appeared to be a greatly increased eagerness on the part of business firms to display their goods to the members of the Congress, but the accommodation provided for exhibitors was manifestly inadequate, and one leading firm was compelled to display a large exhibit in the sample room of a hotel. It was really the anxiety of this firm to avoid a repetition of their experience which prompted them, early in the year following the Adelaide Session, to make application to the Congress Committee for permission to exhibit at the Melbourne Session. This application was soon followed by inquiries from others, and it led to the early formation of three small sub-committees to deal with pharmaceutical preparations and foods, surgical instruments, and pathological exhibits, respectively. These three sub-committees were later welded into a Museum Committee, and constituted a Sectional Committee, with its own secretary and convener.

The first problem which confronted the Committee was the acquiring of sufficient space for a good exhibition. In this matter the Committee is grateful to many members of the professorial staff of the University, who kindly placed the rooms in their various departments unreservedly at the disposal of Congress. It was finally decided to accept the generous offer of Professors Allen and Berry, and to organize a compact exhibition and museum under the one roof of the New Medical School. The opportunity was most favorable, as the rooms were for the most part quite new, in one instance the room had never been tenanted, and all were beautifully lighted.

Although sufficient space was thus obtained, the Committee did not feel justified in issuing any public invitation to exhibitors. It was soon found that business firms were quite alert in order to make application for leave to exhibit. In each instance, after the Committee had approved of the exhibitor, he was so notified by the secretary, and invited to make application

for a definite amount of floor space. It was found possible to accord to each exhibitor the full amount of space he desired, and to avoid both overcrowding and sparseness of the exhibits.

Later, a printed circular was sent to each approved exhibitor, bearing full particulars as to the dates for the moving in of exhibits, the opening of the Congress, the measurements of doorways and stairways, the capacity of lifts, and the conditions to be observed by exhibitors in the interests of the Medical School buildings.

Shortly before the Exhibition opened each exhibitor was required to submit to the Museum Committee a complete list of articles proposed to be exhibited, and the Committee rigidly excluded patent and proprietary articles advertised to the public, and reserved the right to veto at any time, a right which, in one or two instances, it was found necessary to enforce.

Through the kind assistance of Dr. Clendinnen, a motor generator was obtained from a city firm at a nominal cost, and the Committee was thus enabled to offer, free of charge to exhibitors, the constant current for the working of electrical exhibits.

An attempt was made to organize a Museum which would embody the results of the work of members of the profession themselves, and of allied pursuits. In this direction a very encouraging measure of success was achieved, and the Committee is grateful to those members of the Congress who contributed to this very interesting section of the Exhibition and Museum.

Original specimens were shown by Dr. Herschel Harris, of Renal Calculus Roentgenography, and by Dr. Herman Lawrence, of Moulages, graphically illustrating diseases of the skin. These two exhibits attracted much attention on account of both their scientific and artistic value. Dr. E. Roberts, of Hobart, lent a collection of antique surgical instruments, including Cruise's original endoscope, of 1865, and instruments of interest through antiquity or otherwise were also lent by Drs. J. W. Barrett, F. J. Clendinnen, R. H. Fetherston, S. S. Argyle, and W. A. Morton. Rare old medical books were lent by Dr. Mary Booth, Mr. G. A. Syme, and Dr. Felix Meyer. A set of botanical specimens of indigenous flora, with accompanying monographs, were sent by Mr. J. H. Maiden, Government Botanist of New South Wales, and by him presented to the University Library. Much more material of this nature is probably available, and in future Sessions an early and energetic canvass of the profession would certainly result in a large display of objects which represent the labours and hobbies of medical men.

The interest of the Congress Exhibition and Museum was greatly enhanced by its proximity to the excellent Museums of the Anatomical and Pathological Departments, which were thrown open by the professors to the Congress. Visitors and old students of the school were interested and impressed by the evidence of the strides that our Medical School has made during recent years. In addition, Professor Berry personally conducted parties of members of the Congress through the Anatomical Museum, and gave much-appreciated demonstrations of its valuable and instructive contents.

The best thanks of the Museum Committee are due to exhibitors, who worthily did their share in making the Exhibition a complete success. If one small regret might be expressed, it is that the press of sectional work and social engagements during the Congress week prevented many members from visiting the Exhibition and Museum, and left little time for the greater number, who saw and appreciated the Exhibition, to devote to it the careful and detailed inspection which it merited.

AUSTRALASIAN MEDICAL CONGRESS.

TRANSACTIONS

OF THE

Eighth Session, Melbourne, October, 1908.

FIRST GENERAL MEETING,

MONDAY, 19TH OCTOBER, 10.30 A.M.,

(WILSON HALL).

THE PRESIDENT, PROFESSOR ALLEN, who was received with acclamation, occupied the Chair, and over 200 members were present.

THE PRESIDENT said that his first duty was to express his heart-felt thanks to the Congress for having elected him to preside over its meetings. He fully appreciated the great responsibilities of the office, but relied on the assistance of the Congress Officers, and on the generous support of members. At the Inaugural Meeting in the evening, he would have an opportunity of speaking further, and at present he would simply call on the General Secretary to submit the Report of the Executive Committee.

THE GENERAL SECRETARY, DR. MAUDSLEY, submitted the following Report, which was taken as read, having already been published in the daily press:—

REPORT OF THE EXECUTIVE COMMITTEE.

At the Final General Meeting of the Seventh Session held in Adelaide, in September, 1905, it was determined "that the next meeting of Congress be held in Melbourne—provisionally—in 1908," and Professor H. B. Allen, M.D., was elected President of the Eighth Session.

In December, 1905, at a meeting of the Presidents of Medical Associations of Victoria, together with the Past President, Sir T. N. Fitzgerald, and the President-elect, it was determined that the General Committee of the Eighth Session should be composed of six members nominated by the Medical Society of Victoria, six by the Victorian Branch of the British Medical Association, three by the Faculty of Medicine in the University of Melbourne, three by the Medical Defence Association of Victoria, three by the Ballarat Division of the Victorian Branch of the British Medical Association, three by the Bendigo Branch of the

Medical Defence Association, three by the Border Medical Association (Victorian members), together with the President of the Medical Board, the Chairman of the Board of Public Health, the Government Medical Officer, the Inspector-General of Hospitals for the Insane, the Director-General of the Army Medical Services of the Commonwealth, the Principal Medical Officer for the Victorian Forces, and the Health Officer of the City of Melbourne, with power to add to their number. The Committee was constituted accordingly, and subsequently exercised its right of co-option freely, till it included 139 members, representing very fully the profession in the metropolitan and extra-metropolitan districts. The General Committee appointed an Executive of 31 members, Mr. G. A. Syme being appointed Treasurer, Dr. H. C. Maudsley General Secretary, and Mr. L. J. Balfour, Dr. H. D. Stephens, and Mr. W. B. Vance Associate Secretaries. The General Secretary obtained leave of absence for a short visit to England, and Dr. A. Jeffreys Wood was appointed Acting General Secretary during his absence. A Reception Committee, an Accommodation Committee, a Congress Dinner Committee, and a Literary Committee were also constituted.

Their Excellencies the successive Governors-General, the Lord Northcote and the Earl of Dudley, graciously granted their patronage, as did also their Excellencies Vice-Admirals Sir Wilmot H. Fawkes and Sir Richard Poore, and their Excellencies the Governors of the Australian States, of New Zealand and Fiji, the Lieutenant-Governor of Victoria, and the Administrator of New Guinea. The Hon. the Premier of Victoria and his Ministry granted their special countenance and support to the Session, and promised that the Transactions of the Session should be published by the Government Printer. Special concessions for travelling were made by the Railway Departments of Australia and New Zealand, and by the various shipping companies. The Council of the University of Melbourne kindly granted the use of the University buildings for the purposes of the Session.

The Presidents of former Sessions of Congress were appointed Vice-Presidents of Congress, and with them were associated the successive Fleet Surgeons, the Director-General of the Army Medical Services of the Commonwealth, and Dr. John Williams.

Nineteen honorary members of Congress were appointed, including the Presidents of certain learned societies, representatives of allied professions, members of the Faculty of Medicine not medical practitioners, and a few others, among whom may be mentioned the Registrar of the Pharmacy Board, Mr. H. Shillinglaw, who has rendered much service as a member of the Museum Committee.

Local Secretaries were appointed for the various States as follow:—For New South Wales, Dr. A. A. Palmer; for the North Island of New Zealand, Mr. Tracy Inglis; for the South Island of New Zealand, Mr. F. Stanley Batchelor; for Queensland, Mr. W. W. R. Love; for South Australia, Mr. J. B. Gunson; for West Australia, Dr. A. J. H. Saw; and for Tasmania, Dr. Gregory Sprott.

Representatives of Governments were accredited as follow:—For New South Wales, Dr. J. Ashburton Thompson; for New Zealand, Dr. J. M. Mason; for Queensland, Colonel John Thomson, P.M.O.; for South Australia, Dr. A. A. Hamilton; and for Tasmania, Dr. J. S. C. Elkington.

The Congress was divided into eleven sections, a section of naval and military medicine and surgery being created in accordance with the resolution adopted at the preceding session in Adelaide. After correspondence with Committees in the various States, a President and three or four Vice-Presidents were appointed for each section from among the profession in the States other than Victoria. Two secretaries were appointed for each section from among the Victorian members, and after careful consideration, Sectional Committees of Victorian members were also created, and these Committees have rendered great service in choosing subjects for discussion in the various sections, and in obtaining writers and speakers on these subjects.

It was determined that meetings of Full Congress should be held to discuss (1) Hospitals in relation to the Medical Profession, and (2) Syphilis; and that addresses in Full Congress should be given by the Presidents of the sections of (a) Medicine, (b) Surgery, (c) Pathology and Bacteriology, (d) Public Health and State Medicine. Special Committees were appointed to arrange debates concerning Hospitals and concerning Syphilis. Another Committee was appointed to organize the Congress Museum. The Executive is most grateful to these Committees for their successful labours.

In accordance with instructions from the Seventh Session, Draft Standing Orders have been prepared for Congress, and will be submitted for your approval; and steps have been taken to secure improvement and uniformity in Medical Statistics, concerning which the report of a special committee will be placed before you. The committee appointed by the Seventh Session to promote union of the medical organisations of Australasia made no progress, owing to many associations failing to appoint representatives; but since that session, the Medical Society of Victoria has entered into the closest possible form of union with the Victorian Branch of the British Medical Association.

As the outcome of very careful discussion, it was resolved that the Eighth Session should be held from Monday, 19th October, to Saturday, 24th October, 1908. It was necessary to choose a time when the University of Melbourne would be in vacation, so that all the lecture rooms would be available, as well as the Wilson Hall and the Conservatorium of music. Midsummer and midwinter were deemed unsuitable. After correspondence, the Universities of Sydney, Otago, and Adelaide kindly resolved to grant leave of absence to University teachers desiring to attend the Congress; the University of Otago adding the condition that provision should be made for the discharge of University duties.

Two circulars have been issued widely to members of the profession throughout Australasia, and draft programmes of Congress work and entertainment, and preliminary drafts of subjects recommended for consideration in the various Sections, have been forwarded, as far as practicable, to members of Congress as they joined. A full programme of the proceedings of Congress has, during the past week, been distributed among the members.

His Excellency the Admiral consented to depute a member of his medical staff to attend the Congress, and the Committee notes with pleasure the active participation of Fleet Surgeon J. Falconer Hall. Invitations were sent to members of the Royal Army Medical Corps serving in India, and to members of the Indian Medical Service, and these were widely circulated through the kindness of the Director-General of the Indian Medical Service. Surgeon-General F. W. Trevor, C.B., V.H.S.,

Principal Medical Officer with the Army in India, has deputed Captain J. F. C. Mackenzie to represent the Royal Army Medical Corps in India.

The Executive has to deplore the death of Sir Thomas FitzGerald, Past President of the Congress, who had taken an active part in the earlier work of the Committee, and the deaths of two members of the General Committee, Dr. J. R. McInerney, of Fitzroy, and Dr. J. A. Reid, of Sale.

The Executive is most grateful to those ladies and gentlemen who have so kindly come forward to assist in entertaining the visiting members of Congress; and also to the Deputy Postmaster-General of Victoria, who has been so good as to provide a special post office, telephone, and telegraph service for the Congress.

The number of members who have joined the Congress is, approximately, 814; of whom 477 are resident in Victoria, 130 in New South Wales, 69 in South Australia, 38 in Queensland, 33 in New Zealand, 25 in Tasmania, 19 in West Australia, 2 in India, 1 in the Gilbert Islands, 1 in England, and 19 are honorary members.

THE PRESIDENT, in proposing that the report of the Executive Committee be adopted, said that the Visitors' Book of His Excellency the Governor would be available at the Governor's Office in the Treasury Building. Dr. Tidswell, President of the Section of Pathology and Bacteriology, would be unable to attend the Congress, by reason of family bereavement. Dr. Wilton Love, one of the Vice-Presidents, had kindly undertaken to act as Chairman of the Section. Professor Welsh, who was a Senior Vice-President, was kept away by illness. There would be no address on Pathology and Bacteriology, and the whole of the afternoon of Friday, 23rd October, would be devoted to the discussion on Syphilis.

DR. MASON (New Zealand), in seconding the motion, spoke of the great development which the Congress was undergoing in its present session, in number of members and in scope and amount of work. The Congress might now join hands with the Congresses of the Old Country. Serious consideration should now be given to the amalgamation of all the medical societies of Australasia, for the betterment of the profession, which always went hand in hand with public safety. (Hear. hear.)

The motion was put and carried.

THE LATE SIR T. N. FITZGERALD.

THE PRESIDENT moved that the Congress place on record its sorrow at the death of Sir T. N. FitzGerald, C.B., F.R.C.S.I., and its sense of the great services rendered by him to the public and the profession; and that a copy of this resolution be sent to Miss FitzGerald.

COLONEL THOMSON (Brisbane), in seconding the motion, said:—I have been invited to second this vote of sympathy, and perhaps it is befitting that I should, for I was with Sir Thomas when he finally ceased from all his labours. You perhaps know the story. He took a winter trip to Queensland waters in search of health. At Brisbane he felt worse than when he started, and would fain have returned home; but he dreaded the long train journey, and found the return steamer accommodation inferior to what he looked for, and so elected to continue his voyage. I joined the boat on Saturday, 4th July, and Sir Thomas was then confined to his cabin. On the Monday, he was on deck for a few

hours ; but on Wednesday his heart failed, and he passed peacefully and silently away. Whatever there may be of poetry, or beauty, or grandeur in death, lay there in that cabin of the *Wyreema* in that northern sea, whose waters lap the coral ridges of the Great Barrier Reef. Many of you, most of you, knew him better than I did. I only met him on some four or five occasions, and then only for a few minutes each time ; but I remember the almost boyish pride I felt when he complimented me on a successful case of double talipes, by the operation he described at the Melbourne Congress of 1889. The name of "Fitz."—I mention it with all reverence, yet it was the kindly way in which he was referred to throughout Australia—was a name to conjure with. He was the Chief Justice of the High Court of Appeal of Australia in all matters surgical, in fact, he was the High Court himself. I cannot do better than quote from an obituary notice of Sir Thomas which lately appeared in the *Australasian Medical Gazette*—a notice whose very strength and truth are owing to its simplicity—"thus passed the greatest surgeon Australia has yet seen, one who was not only a great surgeon, but a great man."

The motion was carried in silence.

JANET LADY CLARKE.

THE PRESIDENT said that, twenty years ago, when the Congress assembled in Melbourne, members were warmly welcomed by the late Sir William Clarke and Lady Clarke. When arrangements were being made for the present Session, Janet Lady Clarke offered to the Committee the use of "Cliveden," her house in East Melbourne, and proposed to take an active interest in Congress proceedings. Severe continued illness had intervened, though he was glad to say that recent reports were favorable. He desired to move, "That the Congress send to Janet Lady Clarke an expression of its sympathy with her in her long illness, and its hope that her health will soon be fully restored."

The motion was seconded by the GENERAL SECRETARY (DR. MAUDSLEY), and carried unanimously.

STANDING ORDERS.

THE TREASURER, MR. G. A. SYME, on behalf of the Executive Committee, submitted a draft code of Standing Orders, which had been prepared in compliance with the request of the Seventh Session in Adelaide. He moved the adoption of section 1 in the following form:—

1. Ordinary members of Congress shall be medical practitioners, legally qualified to practise in their respective countries, who have paid their subscription to the Treasurer of Congress or to the Corresponding State Secretary.

THE HON. G. H. BUTLER (Tasmania) seconded the motion.

DR. R. WORRALL (New South Wales) moved, as an amendment, to omit all words after the word "be," with a view to insert the words "members of the Branch of the British Medical Association in their State, or, in the case of Tasmania, members of the Medical Defence Association of Tasmania." He urged that the time had come when the constitution of the Congress should be changed, and when the Sessions should become meetings of the Australian Branches of the British Medical Association. The Congress would then be able to act effectively in the directions desired by the Branches in the several States.

COLONEL THOMSON (Queensland), supporting the amendment, pointed out that, under the Standing Order as drafted, American practitioners who would not be allowed to register in Brisbane could become members of Congress.

DR. SPROTT (Tasmania) moved, as a second amendment, to omit the words "to practise in their respective countries," with a view to insert instead the words "and registrable in the States of the Commonwealth, the Dominion of New Zealand, or under the Medical Council of Great Britain." He thought the reference to "countries" was too wide, that there should be express recognition of persons registrable in the States of Australia or in New Zealand, and that there should be recognition of those who were included in the British Medical Register.

THE PRESIDENT said that, under cover of the proposed Standing Orders, a very large question had been introduced, amounting to a complete change in the constitution of the Congress. Hitherto membership had been open to all registered medical practitioners in Australasia, except the few whose presence would be objectionable to the large majority. Dr. Worrall's proposal, if adopted, would limit membership to the minority who were members of the British Medical Association. It would lessen the sympathy of the Governments and the interest of the public in the proceedings of Congress. In his own opinion, it would greatly impair the usefulness of the Congress for the very purposes which Dr. Worrall had so much at heart. Under the existing constitution, the Congress served to bring together the great mass of reputable practitioners and to convince them of their common interests, and thus it acted as a most important feeder to the branches of the British Medical Association. No one aware of the action taken by him in securing the amalgamation of the Medical Society of Victoria with the Victorian Branch of the British Medical Association would suspect him of any illwill to the further development of the Association, but he was convinced that to narrow the area of membership of Congress would not be for the welfare of the Congress or of the Association. Some States, such as Tasmania, had no branch of the Association. In the second place, he was unwilling to restrict the avenue of visiting practitioners from Europe or America to the Congress. He trusted that, as the Congress grew in importance, there would be more and more a representation of the profession from the older and larger centres of medical science. But to remove doubt concerning the Australian States, he would move as a third amendment that the words "States or" be inserted before the word "countries."

THE TREASURER (MR. G. A. SYME) seconded the amendment moved by the President. He thought that the Congress could be best administered on its present wide basis. No one had more strongly supported the British Medical Association in Australia than he had, but it was for the Association to induce every member of the profession to become a member of it, in which event the necessity for Dr. Worrall's amendment would disappear. The financial aspect of the matter must not be forgotten, for with greatly reduced membership the Congress would be unable to meet certain contingencies. At present it relied on Government assistance for the publication of its transactions. Under the second Standing Order, the Executive Committee had the power to refuse to admit to membership any practitioner who was deemed undesirable.

MR. A. L. KENNY said that in Victoria only 400 practitioners out of 1,200 were members of the British Medical Association.

After further discussion, the amendment proposed by the President was put and carried; that proposed by Dr. Sprott was withdrawn; that proposed by Dr. Worrall was negatived; and Standing Order 1 as amended was adopted.

On the motion of the HON. G. H. BUTLER (Tasmania) Standing Order 8 was amended by substituting the word "year" for the word "date." The code of Standing Orders as amended was adopted on the motion of the Treasurer, MR. G. A. SYME, seconded by the HON. G. H. BUTLER, in the following form:—

AUSTRALASIAN MEDICAL CONGRESS.

Standing Orders.

1. Ordinary Members of Congress shall be medical practitioners legally qualified to practise in their respective States or countries, who have paid their subscriptions to the Treasurer of Congress or to the Corresponding State Secretary.

2. The Executive Committee reserves the right to refuse or to return any subscription.

3. The Subscription shall be determined for each Session by the Executive Committee thereof, and shall be payable to the Treasurer or State Secretary, and shall entitle the member to a copy of the published transactions of the Congress, if any.

4. Honorary members, not necessarily legally qualified medical practitioners, may be elected by the Executive Committee.

5. There shall be a Preliminary General Business Meeting and an Inaugural General Meeting of Congress on the first day, and a Final General Business Meeting on the last day of the Session. Other General Meetings may be held in pursuance of resolution of the Congress or of the Executive Committee.

6. At the Preliminary General Business Meeting, the report of the Executive Committee shall be submitted, and also the report of the Treasurer to the preceding Session. Other business shall be as arranged by the Executive Committee. Resolutions may be submitted by members of Congress, but only after notice of motion, which must be in the hands of the General Secretary at least a week before the first day of the Session.

7. The Inaugural General Meeting shall be devoted entirely to the usual addresses and resolutions. The only speakers shall be those designated by the Executive Committee.

8. At the Final General Business Meeting, the place and year of the next Session of Congress shall be determined, and the President for that Session elected. Resolutions carried in any Section or Sections and ordered by any Section to be reported to the Congress shall be considered. Other resolutions carried in Section shall not be reported. Motions may be submitted on behalf of the Executive Committee. Other motions, of which at least twenty-four hours' notice has been given, may be submitted. The President shall have full power to restrict debate.

9. Subject to any resolution of Congress, the business at other general meetings shall be arranged by the Executive Committee, which may, as far as it deems desirable, designate the readers of papers and the speakers.

10. The proceedings of each Section shall be regulated by the President of the Section at his discretion, provided that a provisional programme of work for each Section may be published by the Executive Committee after consultation with the Sectional Officers. Two or more Sections may meet together for any joint work.

11. The Executive Committee shall determine which addresses, if any, by President of Section shall be given in full Congress, and which shall be given in the Section. With the exception of addresses to be given in full Congress, the address in any Section may at the discretion of the President of Section be replaced by a paper or by the opening of a discussion by the President of Section.

12. The President of the Congress shall preside at all General Meetings of the Congress unless the Executive Committee shall with his consent have arranged otherwise. In the absence of the President, the Past President of longest standing present shall preside. If this provision fails, the meeting shall elect a Chairman.

13. In Sectional Meetings, in the absence of the President of the Section, the available Vice-President, whose name is highest in the list of Vice-Presidents, shall take the chair. If this provision fails, the Section shall elect a Chairman.

14. Any debate in General or Sectional Meeting may be interrupted by motion proposed and seconded that the question be now put, or that the meeting pass on to the next business. The motion in either case shall be put without further debate, and, if it is carried, the question shall be put, or the next business called on, as the case may be. Otherwise the decision of the President or Chairman in the conduct of any discussion shall be final.

15. Unless expressly provided otherwise by the Executive Committee, no paper nor report may occupy more than twenty minutes. No speaker in any discussion may occupy more than ten minutes. The author of any paper or report shall have the right of reply not exceeding five minutes.

16. Notice of all papers, exhibits, &c., to be submitted should be in the hands of the General Secretary or Secretary of Section two months before the opening of Congress, otherwise they will probably not be included in the published programme of Sectional work. Papers may, however, be accepted up to and even during the meeting of Congress. All notices of papers, exhibits, &c., are subject to acceptance by the Executive Committee, or, in emergency, by the President of the Section concerned.

17. Reports prepared at the request of the Executive Committee or of any Sectional Committee should be in the hands of the General Secretary or Secretary of Section at least one month before the opening of Congress, and as far as possible such reports will be printed for distribution at or before the Congress.

18. Short abstracts of papers should be forwarded to the General Secretary or Secretary of Section one month before the opening of the Congress, and as far as possible such abstracts will be printed for distribution at or before the Congress.

19. Members detained in other Sections may forward to the Secretary of any Section a brief note to be incorporated in any discussion or in the report thereof. Such note must reach the Secretary before the close of the discussion in question.

20. All reports or papers submitted to any meeting of Congress shall be handed to the General Secretary or the Secretary of Section before the close of that meeting. They may not be subsequently altered without the consent of the Executive Committee.

21. No vote of thanks shall be proposed to the author of any paper.

22. The Executive Committee reserves the right to determine for itself or by any Committee appointed for the purpose what reports, papers, or speeches, or what parts of reports, papers, or speeches shall be published in the Transactions of the Congress.

REPORT OF COMMITTEE CONCERNING MEDICAL STATISTICS.

THE PRESIDENT then called upon Dr. W. P. Norris to submit the Report on Medical Statistics, which had been prepared by a committee, consisting of Dr. W. P. Norris (convener), Dr. J. Jamieson, and Dr. J. W. Springthorpe, with the cooperation of Mr. G. H. Knibbs, the Commonwealth Statistician.

DR. W. P. NORRIS then submitted the following Report:—

THE REPORT OF THE SPECIAL COMMITTEE APPOINTED TO CONFER WITH THE
FEDERAL AND STATE STATISTS ON THE SUBJECT OF UNIFORMITY IN THE
VITAL STATISTICS THROUGHOUT THE COMMONWEALTH AND NEW
ZEALAND.

The Committee met shortly after its appointment, and took as the basis of its deliberations the recommendations of the Conference of Statists held in Hobart in 1902.

These recommendations dealt with the chief data which would be of value in framing comprehensive vital statistics, and regarding which at least there should be uniformity.

In view of the fact that shortly after the Committee was constituted, a Conference, called by the Federal Statist, was to be held, at which all the State Statists were to be present, a memorandum on the subject of uniformity, and containing recommendations as to other cognate matters, was drawn up by the Committee and addressed to the Conference, which courteously received it and gave it consideration.

We are glad to be able to report to the General Committee that uniformity has now practically been reached in the statistical data collected throughout Australasia, though with one important departure from the recommendations of the Hobart Conference, viz., in the classification of causes of death.

The Nosological Index drawn up by Bertillon was substituted for that adopted by the Registrar-General of England. This was the result of the strong arguments put forward by Mr. Knibbs, the Federal Statist, who had also been good enough to forward to the Committee an advance proof copy of the translation of the Index, which he proposed to place before the Conference.

It is, of course, essentially a medical question whether or not the allotment of diseases to the various headings in any Index is strictly in accordance with medical science, and a short study of the classification then in use indicated that it did not reflect contemporary medical science. Its use certainly had the advantage of facilitating the comparison between England and those of her Colonies which tabulate their death statistics according to it. On the other hand, the Internationality of Science suggested the desirability of broadening the basis of comparison in vital statistics by the universal adoption of the same nosological index, and a survey of the facts regarding the Bertillon Tabulation showed that it is being adopted more widely year by year. France, the United States, and Canada, with other countries, already use it, and, as through an International Statistical Institute permanent provision has been made for the repeated revision of this Index, the Committee felt justified in indicating to the Conference its preference for the Bertillon tabulation.

The Conference, after consideration, unanimously approved of the adoption of Bertillon's Nomenclature and Nosological Index, as revised in 1900, with an understanding that recommendations should be made for consideration at the revision to be undertaken in 1909.

Your Committee has conferred with Mr. Knibbs, the Federal Statist, regarding a list of causes of death (and disease)—not mentioned in the present Bertillon classification. These, with suggestions as to where they should be placed, are appended to this report. Mr. Knibbs will forward the list with others to the Central Revision Committee.

With regard to individual factors in the collection of the vital statistics, the Committee is of opinion that under existing conditions throughout the Commonwealth, with rapid changes in local population caused by migration, changes of occupation, and the like, a decennial census is too infrequent, and that for the collection of certain data, to be specified by the Statists concerned, the census should be quinquennial, and simultaneous throughout the Commonwealth and New Zealand.

In regard to *Births*, the Committee is of opinion that it would be of advantage if the births occurring in each month were tabulated, and also if, as opportunity offers, the factors which may exercise a causative influence on the sex of children were investigated. The Commonwealth would thus furnish its quota to the statistical investigation of the problem of masculinity, the determination of sex.

Regarding deaths, the Committee considers that efforts should be made to facilitate uniform certification by medical men, so that all would certify the same disease in the same way, and, wherever possible, state a specific cause, and not a general cause of death. A uniform form of certificate requiring certain particulars to be given might assist in securing these desirable ends, and the Committee would commend to the consideration of the Congress a form submitted by the Federal Statist. Copy attached.

The investigation of infant mortality by statistics could with advantage be extended beyond the first to the end of the second year of life.

It is desirable that in mortality statistics count should be taken of such factors as the month of the year, occupation, and, where practicable, of other matters of social importance.

The Committee desires to point out that there is practically no investigation into the ratios of sickness and accidents. This is a question of great economic importance as well as of vital interest to the science of sanitation. At present such data can only be obtained from the private records of Friendly Societies, and the records of the more important infectious diseases obtained under the various Public Health Acts. The Committee does not feel justified at this juncture in recommending the Congress to advise an extension of the demands on medical men, but yet wishes to place on record its opinion of the value of such records.

The Committee would also urge that it would be well for the Congress to indicate its realization of the importance of the function of the medical profession in regard to vital statistics, and especially their responsibility in the matter of mortality records.

A nomenclature such as that now issued by the Federal Statist and available to all medical practitioners should prove of great service in this direction; and the issue of uniform death certificates, which would be greatly facilitated if the General Committee can see its way to indorse the principle, and so strengthen the hands of the Statists, would render the task of the rank and file of the profession still easier.

W. PERRIN NORRIS,
Chairman of the Committee.

APPENDIX I.

Suggested Resolutions for the Congress.

1. Adopt the Report.
2. That, in the opinion of the Congress, a form of death certificate which would facilitate uniform and specific certification of the causes of death is most desirable, and any such certificate should be forwarded direct to the local registrar of deaths, who should issue a burial certificate, and that the various Government Statists be informed of the opinion of the Congress.
3. That a copy of the other recommendations of the Committee be forwarded to the statists for their consideration.

APPENDIX II.

Suggested form of Death Certificate.

The cause of ^{his}
_{her} death was as hereunder written :—

Cause of Death.	Duration of each cause.†			
	Years.	Calendar Months.	Days.	Hours.
(1) Disease causing Death *				
(2) Immediate Cause of Death				
(3) Complications and Contributory Causes, if any				

NOTE.—Neither (2) nor (3) need be filled in if doubt exists as to the item.

Confirmed by }
Not confirmed by } Post-Mortem.

* In Death by Violence state—(1) Mode of injury, accidental, suicidal, or homicidal; (2) Nature of the injury and immediate cause of death; (3) Contributory causes or conditions.

† The duration of each form of disease or symptom is reckoned from its commencement until death occurs.

DR. NORRIS moved the adoption of the resolutions contained in Appendix I. to the Report.

DR. ELKINGTON (Tasmania), in seconding the motion, said that those who in old days had endeavoured to take out from the varying forms any statistical matter to be applied to Australia would realize the improvement that had come from the work and system of the Commonwealth Statistician, Mr. G. H. Knibbs.

On the motion of DR. E. L. CROWTHER (Tasmania), seconded by DR. F. C. BATCHELOR (New Zealand), it was resolved to refer the Report to the Section of Public Health and State Medicine to consider and report thereon at the Final General Meeting, on Saturday, 24th October. It was agreed that the suggested form of death certificate should be printed and circulated.

AMALGAMATION OF MEDICAL ASSOCIATIONS.

DR. G. E. RENNIE (New South Wales) wrote regretting that he could not arrive in Melbourne in time to personally present a Report in pursuance of the resolution adopted at the Seventh Session of Congress, held in Adelaide, but enclosing the following documents, which were read by the President:—

1. Circular Letter to Associations.

25 Angas-street, Adelaide.

DEAR SIR,

The following resolution was passed in the Section of State Medicine and Medical Ethics at the recent Congress:—

Proposed by Dr. Rennie (Sydney), seconded by Dr. Beeston (Newcastle)—

"That a Committee be appointed, consisting of representatives from each State, with power to add to their number, to consider the best means to be adopted to amalgamate the various medical organisations throughout Australia and New Zealand, two representatives to be elected by the local branches of the British Medical Association in each State, by the Victorian Medical Society, and, in Tasmania, by the Medical Section of the Royal Society, and by each Medical Defence Association in Australasia. That the Secretary of the Section of State Medicine and Medical Ethics should communicate the above resolution to the various bodies, with a request to send him the names of their representatives.

"That the Committee bring up their report to next Congress."

Will you kindly bring this resolution under the notice of your society.

Yours faithfully,

ARCH. A. HAMILTON,
Secretary of Section of State Medicine and Medical Ethics.

2. Letter from the Secretary of the Section of State Medicine and Medical Ethics (Adelaide).

1st September, 1906.

DEAR DR. MAUDSLEY,

At the recent Congress a resolution, of which I enclose a copy, was passed in the Section of State Medicine and Medical Ethics.

As Secretary of the Section I carried out the instructions in that resolution.

As I have now no longer any official existence it has been suggested to me that I should forward a statement of what has been done to the Executive of the forthcoming Congress:—

<i>South Australia</i>	-	British Medical Association: Drs. Hayward and Lendon. Medical Defence Association: Drs. Swift and Cavenagh Mainwaring.
<i>Victoria</i>	-	British Medical Association. Decline to appoint. Medical Society of Victoria: Drs. Jeffreys Wood and M. U. O'Sullivan. British Medical Association (Ballarat): Drs. J. F. Mitchell and Steell. British Medical Association (Goulburn Valley). No reply. Medical Defence Association of Victoria. No reply.
<i>New South Wales</i>	-	British Medical Association. "Feel unable to appoint." New South Wales Medical Union. Refused to appoint.
<i>Queensland</i>	-	British Medical Association: Drs. Turner and Brockway. Medical Defence Society: Drs. Taylor and Robertson.
<i>Tasmania</i>	-	Medical Section of Royal Society of Tasmania. Decline to appoint.
<i>West Australia</i>	-	Medical Association of West Australia. No reply.
<i>New Zealand</i>	-	British Medical Association. No reply. Medical Defence Union. No reply.

From the above you will see that the desire for amalgamation does not seem to be either keen or universal.

Yours very sincerely,

ARCH. A. HAMILTON.

3. Report by Dr. G. E. Rennie (*New South Wales*).

159 Macquarie-street North, Sydney,
12th October, 1908.

The resolution moved by me at the Adelaide Congress *re* Amalgamation of Medical Societies was the result of consultation with several members then present. The idea was to formulate some scheme for uniformity of action in the different States on questions of contract practice, hospital abuse, medical defence, &c. But some of the members of the Council of the New South Wales Branch of the British Medical Association, fearing that this meant the possible formation of an Australasian Medical Association, considered that the British Medical Association could take no part in any such movement, and so declined to appoint any representative on the Committee.

The amalgamation of the Medical Society of Victoria and the Victorian Branch of the British Medical Association since accomplished has cleared the air considerably, and at present there seems to be no necessity to take any further steps to carry out this resolution.

On the motion of DR. A. JEFFREYS WOOD, the report so submitted was adopted.

FINANCIAL REPORT OF SEVENTH SESSION.

In the absence of Dr. Hayward, Treasurer of the Seventh Session, the Treasurer, Mr. G. A. SYME, read the following financial report of the Seventh Session, held at Adelaide:—

Australasian Medical Congress.—Adelaide Session, 1905.

EXPENDITURE.			REVENUE ACCOUNT.			REVENUE.		
	£	s. d.		£	s. d.		£	s. d.
Expenses of Congress	...	32 15 0	Subscriptions—					
Postages	...	32 7 10	388 members at £1 1 0	...	407 8 0			
Advertising	...	14 17 6	Remittance from Hobart					
Printing	...	116 16 0	Congress	...	138 16 5			
Sundries	...	2 13 0	Interest, Savings Bank	...	20 9 7			
Exchanges	...	1 17 0	Sale of desk	...	10 0 0			
State expenses	...	15 0 1	Sale of copy of transactions	1	0 0			
Typewriting	...	4 12 8						
Cost of desk	...	14 2 0						
Assistance, Secretary	...	5 4 11						
Shorthand reports	...	0 16 0						
Public lectures	...	7 15 5						
Sectional expenscs	...	9 0 1						
Honorarium (C. R. Hodge)	15	15 0						
Remitted to Melbourne—								
June, 1906	£100	0 0						
December, 1907	£195	0 0						
		295 0 0						
		£577 14 0						
(Sig.)								
W. J. HAYWARD, Hon. Treasurer.			Audited and found correct—					
Adelaide, 10th December, 1907.			(Sig.) THO. C. WALKER, F.I.A.S.A.,					
			Public Accountant.					

On the motion of MR. G. A. SYME, seconded by PROFESSOR E. C. STIRLING, the report was adopted.

TROPICAL AUSTRALIA.

DR. R. ARTHUR (*New South Wales*), who was unable to reach Melbourne that morning, gave notice of the following motion, "That a deputation from this Congress wait upon the Prime Minister and request

that a Royal Commission be appointed to inquire into the question of the possibility of the permanent colonization of Tropical Australia by the Anglo-Saxon and other white races."

On the suggestion of the Executive Committee, conveyed by the President, and with Dr. Arthur's concurrence, the motion was referred to the Section of Public Health and State Medicine, to consider and to report thereon at the Final General Meeting on Saturday, 24th October.

CONTRACT PRACTICE ABUSES.

DR. CLARENCE REID (New South Wales), on behalf of Dr. Adam Dick (New South Wales) and Dr. A. Palmer (New South Wales), moved a series of resolutions concerning Contract Practice Abuses.

It was resolved that the resolutions be printed and circulated, and be considered at the General Meeting of Congress on Wednesday morning, 21st October, after the discussion concerning the Relations of the Medical Profession to Public Hospitals.

ADJOURNMENT.

THE PRESIDENT then declared the Congress adjourned till the Inaugural Meeting in the Melbourne Town Hall, at 8 o'clock p.m.

INAUGURAL MEETING.

MONDAY, 19TH OCTOBER, 1908, 8 P.M.

(TOWN HALL, MELBOURNE.)

The formal opening of the Eighth Session took place in the Melbourne Town Hall on Monday evening, 19th October. There was a brilliant audience numbering about 2,500. On the platform the President, Professor H. B. Allen, occupied the chair, having on his right His Excellency the Governor of Victoria (Sir Thomas Gibson-Carmichael, Bart.), the Minister of Water Supply (the Hon. G. Swinburne) representing the Premier and Cabinet of Victoria, the Lord Mayor (Sir H. Weedon), Dr. F. C. Batchelor (President of the Dunedin Session), the Hon. Dr. G. H. Butler (President of the Hobart Session), Fleet Surgeon J. Falconer Hall, Dr. John Williams, Dr. M. J. Symons (Dean of the Faculty of Medicine in the University of Adelaide), Dr. A. J. Wood, Mr. G. A. Syme, Mr. L. J. Balfour, and the Hon. Victor Hood. On the left of the chair were the Lieutenant-Governor (Sir John Madden, G.C.M.G.), the Speaker (the Hon. Frank Madden), Colonel John Thomson (President of the Brisbane Session), Professor E. C. Stirling, F.R.S., (President of the Seventh Session at Adelaide), Surgeon-General Williams, Professor Anderson Stuart (Dean of the Faculty of Medicine in the University of Sydney),

Lieut.-Colonel Fetherston, Dr. H. C. Maudsley, Mr. W. B. Vance, and Dr. H. G. Stephens. Lady Gibson-Carmichael was also present. Among the audience were the Chief Justice of the High Court of Australia (Sir Samuel Griffiths, G.C.M.G.), the Attorney-General of the Commonwealth (the Hon. L. E. Groom), and the Postmaster-General (the Hon. S. Mauger). The South Gallery was filled with members of the nursing staffs of the Metropolitan Hospitals, and the Organ Gallery was occupied by the medical students of the University of Melbourne. Prior to the opening ceremony, an organ recital was given by Dr. W. G. Price, City Organist.

THE PRESIDENT said that his first duty was to request His Excellency the Governor to be so kind as to declare open the Eighth Session of the Australasian Medical Congress. (Cheers).

THE GOVERNOR'S OPENING SPEECH.

HIS EXCELLENCY THE GOVERNOR (SIR THOMAS GIBSON-CARMICHAEL) spoke as follows:—Professor Allen, Ladies and Gentlemen—In declaring this Congress open, it is, I believe, my duty to say a few words of welcome to the members of the Congress on their coming to this State, in which I have the honour to represent His Majesty. My words shall not be many; but I trust that the members of the Congress will not think that because I do not say much I do not feel very warmly towards them. There are numerous reasons why we should welcome you to this State. One of them, perhaps you may think it a selfish one, is that by your Congress you give to us an opportunity for realizing how much science has done and is doing for the good of mankind. I have not been long in Victoria, and so perhaps I am not entitled to speak; but I am afraid that there are not very many people in Victoria learned in medical matters. I, at any rate, am one that you would look on as very ignorant. Probably there are not a very large number of the inhabitants of this State who would even understand the papers which you will read in your various sections. I feel sure there are not a very large number who would take an intelligent part in discussing those papers; but, for all that, there are probably very few who do not realize to some extent how much is due to science of what helps to make life easier or happier for us. There seems to me to be nothing which is more striking in life at the present day than the way in which the sum of human knowledge is being advanced daily by men, many perhaps members of your Congress, who are working patiently at scientific research and scientific observation. Nothing is more striking, unless that at the same time there are many men who have no pretence to be scientific men, but yet are applying the knowledge gained by scientific observation to common every-day life. Looking to the future, there is nothing more hopeful than the feeling that the co-operation between those who are advancing science and those who are applying it is daily growing closer and closer. In every department of human knowledge and in every branch of industry we find the same thing. A practical application of science is the only way to attain to progress. We gladly recognise that it is so, and that those who have to do the common every-day work of this world are realising it more and more clearly. In nothing is this more obvious than in all that has to do with health, whether mental or physical. Politicians and social reformers may scheme and try to do much, and I trust that they may succeed, but their success can only be insured if they base it on the facts which have been learnt in patient observation by men of science. It appears to me that, to you who are

members of this Congress, there must be a pleasure in recognising how it is to men like yourselves, engaged in research, that any one who thinks at all is daily turning more and more for guidance, in the belief that by such guidance progress can still be insured to our race. This should not only be a stimulus to you, but is no doubt the reason why you should from time to time hold Congresses, where you are able to put in words, more or less to be grasped by unscientific people, the results of your observations. At any rate, that is a reason why men like myself, who have no scientific attainments, but who do wish to see progress in our midst, should welcome you when you come to hold your Congress here. (Cheers.)

THE PRESIDENT then called upon the Hon. Mr. Swinburne, representing the Premier and Cabinet of Victoria.

WELCOME BY THE GOVERNMENT OF VICTORIA.

MR. SWINBURNE said—Your Excellency, Mr. President, Ladies and Gentlemen—I can assure you it gives me very great pleasure, on behalf of the Government of Victoria, to welcome the delegates from the various parts, not only from Australia, but distant lands, to the Medical Congress which is to be held in our State. I must express regret at the absence of the Premier to-night. He is unavoidably detained, and I must express regret at your not being able to listen to one of his characteristic speeches of welcome, which he would certainly have given you. (Hear, hear!) The Government wishes to express its great sympathy with the development of medical science. As a Government, we feel very anxious to do everything we can to promote research in connexion with education in medicine, surgery, and science. We recognise that the Government, as the chief instrument in education, has to do a great deal more than has been done in the past. (Cheers.) That instead of leaving a great deal to private gifts, the Government has to step in and provide funds in order to establish laboratories and funds for research, so that developments may take place worthy of the State and worthy of the Commonwealth. (Hear, hear!) We feel also, as the chief instrument in education, that we cannot lag long behind the Old Country, which has commenced right down at the basis of medical investigation, even in the State schools of England. We feel that before long the Government here will have to initiate some inspection in connexion with the young people of the State, so that they can receive every benefit, even if they are poor. The Government earnestly trusts that the discussions that will take place during your meetings in Melbourne will be worthy of the high place which the Congress discussions have occupied in the past. I feel sure that a great deal will be elucidated, and that members of the medical profession in Australasia will continue in the high place they have occupied in the medical profession in the world. I trust that in your discussions you will keep in mind the noble task you have before you, that medical science may and will lighten the burdens of humanity, and that we will have among our medical men some who will be worthy to follow in the steps of Jenner, Lister, and Pasteur. I wish you every success in your discussions. (Cheers.)

THE PRESIDENT then called upon the Lord Mayor of Melbourne.

THE MUNICIPAL WELCOME.

THE LORD MAYOR OF MELBOURNE (SIR HENRY WEEDON) said—Your Excellency, Professor Allen, Ladies and Gentlemen—I desire, on behalf of

the citizens of Melbourne, to extend a very cordial and hearty welcome to the distinguished visiting members of the Medical Congress, and I feel sure that the deliberations that will take place, and the interchange of ideas, must conduce to the betterment of humanity. I can very inadequately express the desire of the citizens of Melbourne that everything will pass off, not only profitably, but pleasantly to those who are visiting our State, and that on their return they will not be sorry for their sojourn with us. We have come for one special object to-night, and that is to see the Congress opened and hear the President's address. I shall therefore content myself with extending a very hearty welcome to the visitors.

PRESIDENT'S ADDRESS.

THE PRESIDENT (PROFESSOR ALLEN) then delivered the following address:—Your Excellency. Members of the Congress, Ladies and Gentlemen—My first duty is to express my heartfelt thanks to the members of Congress for making me their President. The honour is great, perhaps the greatest the medical profession can confer, and it is the more welcome as it came at the primary instance of the heads of the various medical associations of my own State. The responsibility is great, for it is no light matter, when medical men have gathered from all parts of Australasia, to provide that their deliberations shall produce the greatest possible good to the public and the profession. Experience gained in past Congresses has removed some difficulties; the collective wisdom of the various Committees has solved others; the executive officers have been unsparing in their efforts; and for what has been left imperfect, we trust to the kind indulgence and the co-operation of the Congress itself.

In the midst of our preparations, we have been called upon to deplore the loss of one universally honoured by the medical profession throughout Australasia, the President of the last Session of Congress held in Melbourne, Sir Thomas Naghten FitzGerald. Endowed by nature with every gift that goes to the making of a great surgeon, appointed full surgeon to the Melbourne Hospital when only twenty-two, soon engaged in a very extensive private practice. Sir Thomas had unparalleled opportunities, of which he made noble use. We mourn him as our leader in this State, as a man of lofty professional ideals, as a great social force, as a public benefactor, as a friend who never wavered, a great surgeon, and a great man.

We have reached the Second Session of the second cycle of our Congress, and can look back with some pride on the records of our doings. The first great problem undertaken by Congress was the treatment of hydatid disease, and though the last word on that subject has not been spoken, the proceedings of Congress constituted a great advance in the right direction. The discussions concerning typhoid fever aided to clear away the remains of ignorance concerning its nature and causation, and materially assisted the great preventive movement, in the success of which we now rejoice. The debates on tubercle did much to spread sounder views of the causation, treatment and prevention of the disease. The work of the Congress on cancer was recognised by high authority as ranking with the most suggestive contributions of its time. Surgical operations, first proposed at our meetings, have been universally accepted, and diagrams illustrating them have been copied from our transactions into the various text books: Such observations might be multiplied indefinitely, but probably the two most far-reaching results of Congress have been less direct.

The first consists in the uplifting of the plane of work in the profession, and the promotion of research. The second lies in the general stimulus to preventive medicine, as manifested in progressive legislation, in the activities of the various Boards of Health, and in every State and municipal and private enterprise forming part of the great contest with death and disease. On another side, the Congress has done much to establish better relations within the profession, to disseminate finer ideals of co-operation, to favour union of practitioners, not only within each State, but throughout Australasia, for the promotion of medical science, and the welfare of those who devote their lives to it.

Nearly twenty years have passed since we last assembled in Melbourne, and within that time the change already in progress in the popular attitude towards Science has become more and more marked. The great generalizations of the conservation of energy and of inorganic and organic evolution have little by little exercised their leavening influence; and experience has begotten a new regard for Truth, not as something unstable, needing blind defence at all hazards, but as a glorious certainty that error cannot touch, that can only be revealed in greater grandeur and clearer vision by the deepest research, the most unfettered thought. We have now no hesitation in viewing the living body as a machine, never generating energy, but ever transforming it, using the hidden force of food for the maintenance of bodily heat and for all the manifold activities of life, till it escapes in degraded form as radiated heat. The engines of the latest greyhound of the Atlantic cannot compare in efficiency with the wing-muscle of a bird. As the astronomer resolves the starry world largely into two great streams of stars moving in opposite directions, yet all made up of similar elements, so the physicist contemplates life in its two great divisions, so like, yet so unlike, the vegetable world by wondrous acts of synthesis creating stores of energy, that animal life may set free for its own purpose, unloosening the bonds so strenuously tightened, converting the secret tensions into the energy of new growth and work.

Life is a chemistry, a bundle of fermentations, a complex of cyclical actions and reactions; and how close is the correspondence between the physics and chemistry of life and not-life. Witness the saturated chemical solution that is inactive till a microscopic crystal is added, when in a few moments the germinal impulse towards specific form spreads through the whole mass; or the injured crystal that heals itself in the mother liquid; or the metallic wire that under repeated tension strengthens itself in point after point that tended to give way; or the finely divided platinum, that by its colloid tension has power like a ferment to cause indefinite oxidation, a power that may be destroyed by processes akin to sterilization, that may sicken, that may be inhibited by infinitesimal doses of poison. How marvellous, too, are the results the chemist has attained in studying the constitution of the actual life stuff. Serum albumin and egg albumin have both been crystallised. Albumose has recently been broken up, by means of water, alcohol and salts, into five different bodies, each obtainable in fairly pure state, with characteristic physical properties, solubilities and reactions. Emil Fischer tells us how, from the amino-acids, a hundred different substances have been manufactured nearly related to peptone. Dimly we begin to discern vague chemical forms of infinite intricacy, where proteid and carbohydrate and hydrocarbon are linked together in loose associations, the partners swaying in and out in endless rhythmical figures, each dissolution followed by a new integration. Such a conception corresponds only too faithfully with the enormous difficulties of biochemical study.

Yet the ultimate contrast between life and not-life remains as sharp as ever. In every chemical union, whether in life or not-life, a new substance is created; but for not-life here the matter ends, whereas every living being continually assimilates dissimilar things to itself, and multiplies in its own likeness. This contrast holds for the simplest unicellular plant or animal as fully as for the highest and most complicate type. It is difficult to realize the magnitude of the power to assimilate and multiply. One observer has estimated that, in the breeding of rotifers, if all the possible progeny could be preserved, there would be in the course of a year a mass of organic matter as large as the known universe. In the transplantation of cancer from one mouse to another, the cells introduced in the first tiny graft have multiplied by continuous division in different animals till the total tissue produced was as large as a St. Bernard dog. The continuity of life is still more wonderful. Apart from the question of the ultimate origin of life from not-life, of which we know nothing, every cell now alive, no matter in what plant or animal, has come down in unbroken living chain from the beginning. Untold myriads of cells have perished, but in the direct line there has been no death. Each cell divides, and its parts again divide; death may end any particular chain; but the life that abides is continuous. This fact is easily grasped in the history of unicellular organisms, such as the lower forms of bacteria, where multiplication by division is simple, without apparent formation of spore, without conjugation of sex. In the higher forms of bacteria, the spore is but a specialised part of the living cell, in which the continuity of descent is focussed. In the more complex forms of vegetable and animal life the body perishes, but in the reproductive cells, with their wonderful reductions to unsatisfied forms and their subsequent conjugation, with all the multiplication that follows till the new individual is complete, with the new specialization of germ cells by reduction and the new conjugation, here, as truly though more abstrusely, life that abides is unbroken. In this sense, parents and offspring are one.

In contrast with this essential continuity, is the constant tendency to individualisation. The chemistry of immunity, with the history of precipitins and lysins, indicates how profound and intimate is the difference between one animal and another, one individual and another. Fundamentally, no two living cells can be exactly alike. Each is the product of a particular inheritance and a particular environment. In a bacterial culture, under laboratory conditions, the variations of environment may be reduced to a minimum, but can never be entirely suppressed. The more complex the organism, the greater the play of environment. With bacteria, such as those of anthrax, brief changes in environment may affect the individual, but not the generations that follow. More profound and continued changes in environment produce results of more permanent nature. If culture is continued for six weeks at 42 degrees or 43 degrees C., no spores form, and the virulence is attenuated, so that inoculation does not produce any outward symptoms even in baby mice or baby guinea pigs. If the altered bacilli are now grown at the most favorable temperature, 37 degrees to 38 degrees C., spores form, but the culture retains the same degree of attenuation. Chemical agencies may act in the same way, as when transmissible variation is produced by culture in carbolized media. The reverse process is even more instructive. Pasteur showed that the weak vaccinal culture may be restored to full toxicity by transfer through very young animals to older animals, and from less resistant to more resistant species. Here the change occurs in successive generations of bacterial cells multiplying within the body, and nourished by the body

fluids. Transmissible variation is not limited to such qualities as the power to form virus. The mode of life may be much more profoundly changed. Neisser and Massini have succeeded in so cultivating the *Bacterium Coli Mutabile*, that some individuals in a culture acquired the power to ferment milk sugar, and this power was transmitted through several generations, and was retained when the organisms were sown on other media.

In higher forms of life, only those acquired properties can be inherited which affect the reproductive cells. But these cells obey the law that all protoplasm bathed by the same lymph must have the same colloid equivalence. They are affected in greater or less degree by toxic conditions of the general tissue fluid. In fact, we are only beginning to understand how the whole body, including germ cells as well as somatic cells, is nourished in full communion of parts. Sir James Paget long ago taught that every organ in its nutrition is complementary to the rest of the body. The history of the evolution of sexual character is the most striking illustration of this doctrine. Recent experiment shows us a little of the underlying chemistry of the process, though our knowledge is still comparatively superficial, and still "one deep calleth unto another." If some of the old form of tuberculin is inoculated into the arms of infants, a local papule with special characters will appear in those who have any form of active tubercle, and even in those who have traces of healed tubercle. If a drop of one per cent. watery solution of purified tuberculin is put under the lower lid of child or adult, inflammation of the conjunctiva follows in those with any form of active tubercle. With skin or eye, the non-tuberculous remain unaffected. This is a relatively simple instance of the mode in which local disease may produce subtle effects in far distant parts of the body. Ancient doctrines of mysterious sympathies have been replaced by knowledge of hormones, of inter-related digestive actions, of responsive leucocytosis, of the relation between the life of the organs and our conscious and unconscious mental life. Such remote actions are in every case the product of a long series of cumulating acquirements, each step being an adaptation to the demands of the environment on the living organism. While, therefore, it is certain that acquired characters are heritable only in proportion as they affect the germ cells, we must remember that no cells, not even germ cells, have a life wholly apart. Those who believe with Lamarck in the inheritance of acquired qualities have been asked if they think that, when a man develops his muscles by exercise, his brother's children will thereby be modified in like manner. Somatic cells and germ cells are brothers in a very different sense, nourished by the same body fluid, through which they act and react upon each other. This is not mere hypothesis, but has a firm foundation in the facts of inherited immunity.

It is universally conceded that evolution is the response of the organism to the demands of environment. If this response does not increase from generation to generation, the weeding out of the irresponsive will be powerless to produce progress. It can at best only prevent retrogression. But if the response is progressive, natural selection, or in other words the elimination of the unfit, becomes a factor of the utmost importance.

Our knowledge of the laws that govern inheritance of parental qualities has been greatly enriched by the re-discovery of the experiments of the Abbé Mendel, and by the work of his followers, many a dark place being illuminated by the theory of dominant and recessive characters, a theory

that accords remarkably with the latest observations of the actual process of fertilization. De Vries, by his experiments on the breeding of *Oenothera*, has gone far to prove that, at least in many cases, development comes at a leap, and not by slow visible cumulation of characters, though doubtless behind the obvious leap there is a hidden cumulation of results, until a critical period of change supervenes. In descent, the family type is more potent than the individual, and the racial type more potent than the family; but the wave of scepticism concerning heredity has almost spent itself, leaving us with more sure knowledge, purged of ancient error or excess. The family histories of polydactylism, of hæmophilia, of gout, of cancer, have long been conspicuous. Recent research shows how far reaching is the hereditary tendency. Nettleship has added to his series of cases of hereditary coralliform cataract a most extraordinary history of congenital night-blindness in a family, residing in the south of France near Montpellier. The pedigree includes 2,121 persons in ten generations since the year 1637, and 135 are known to have been night blind. Biometric methods tend to show that mental characters obey the same laws of descent as physical. But the doctrine of evolution has become independent of particular methods of interpretation. Whatever weight we may give to elimination of the unfit or to progressive adaptation, however we may view the struggle for existence, the central truth stands firm, and for us needs little further warrant than the fact that every living being in its embryonic development displays in brief the story of its evolution.

If there is still a radical division of opinion concerning the hereditary factor of individuality, there is full agreement concerning the other factor, which consists in the environment of each new life from its first inception. Before birth, after birth, streams of influence are flowing in upon it. Now it thrives in favoring conditions, now it is stunted by mal-nutrition, now it is poisoned by toxines that excite no countervailing reaction, now it adapts itself to conditions that at first promised disaster; every impression leaves its mark; so that the disciples of Weissmann, who deny the transmission of acquired qualities, proclaim the more forcibly the importance of physical, mental and moral culture. In proportion as man holds off from man the unsparing law of elimination of the unfit, there should be more constant, more intense, more enlightened education of every faculty, for if the whip of suffering is removed, the downward course is all too easy, all too sure.

When we attempt to grasp the nature of Life, we find ourselves in presence of a great mystery. Comte long ago remarked that living beings are better known by us in proportion as they are more complex. Our greatest certainties are those of our own consciousness. The imaginings of poet, musician, architect, shadowing forth tragedy, sonata, cathedral, are not mere transformations of physical energy. Bald materialism is possible only by abstraction of essential facts. As Gissing says in *Henry Ryecroft*, "rather I would think that despair at an insoluble problem, and perhaps impatience with those who pretend to solve it, bring about a resolute disregard of everything beyond the physical fact, and so at length a self-deception which seems obtuseness." Hence it is cheering to find, as the motto of one of the most popular manuals of physiology, the saying of John Hunter that life produces organisation, but organisation cannot produce life. Nevertheless, the phenomena of mind are all based upon sensation and reaction to sensation, and these

again on the primitive attractions and repulsions seen in the simplest unicellular organisms. Desire and will were foreshadowed in the first living element. Yet it is difficult, if not impossible, to avoid the belief that Life came out of Not-Life, or at least arose amidst Not-Life. Was Leibnitz then right in contending that nothing is dead, that life is universal? Is not Meredith nearer the truth when he sings of Earth

"She being Spirit in her clods."

The phenomena of radio-activity and the development of electrophysical theory have broken down the bounds between matter and energy. Philosophy long ago proclaimed that matter became nothing if all its qualities were abstracted, and now it is a wide-spread belief that matter is but focalized energy, so that the evolution of matter, inorganic or organic, becomes the evolution of symmetric foci of energy. Shall we shrink from this marvel of mathematics? What are all the glories of light and colour, in sunset and sunrise, and in all the wonders of earth and sea? Outside of living beings, they are vibrations of ether, which we interpret as colour according to the rate of vibration. What is music but our interpretation of aerial vibrations, which we appreciate as melody and harmony according to an exact mathematic. Let us rather hold that physical energy is in the last resort the expression of the Divine will, that the physical and the spiritual are not heterogeneous in the sense that one cannot act upon the other, that in every manifestation of energy the physical and the spiritual are both present, and that the history of the evolution of life has been the specialization of organs through which the celestial mathematic is interpreted more and more fully in finite consciousness. If we believe in the Divine Immanence, we must hold that Immanence is perfect in all things in accordance with their power of containing it; and with growing capacity, who shall limit the new Synthesis? But we do well to remember that Meredith, who proclaims the universality of the spiritual, tells us also as to the higher knowledge of Earth

"For the road to her soul is the Real,"

and how, in his vision, there is no room for superstition, or partial variation, or any of the vagaries of pseudo-science, for

"Around the ancient track marched rank on rank
The army of unalterable law."

With such conception of life, however shadowy it may be, we can base the ideal firmly on the real, or the real on the ideal, knowing that the one cannot be estranged from the other; and we may thus attain a more solid comprehension of the meaning of evolution, of individual development, and of personal, family, social and national duty.

The medical profession has for its special function the culture of human life, and it is well that from time to time we should seriously ask ourselves how this great trust is being discharged. Such a survey, however imperfect, must aid us to realize the vast extent of our responsibilities, entering into every part of life—birth, infancy, childhood, adolescence, maturity, decline, old age and death. How many images these words evoke, how complex the problems that are involved. It is scarcely necessary to say that the medical profession is only one of the agencies concerned, and that, whether changes are for better or worse, only part of the praise or blame can attach to it.

The first question that fronts us is the unsatisfactory state of maternal mortalities. Professor von Herff states that, at his own hospital in Basle,

among 6,000 cases, not one death occurred from puerperal fever contracted in the hospital, and only five cases in all died from puerperal fever, every one of these being infected before admission. Nearly 82 per cent. of his cases had no rise of temperature. Australasian maternity hospitals in various degrees approach towards this record. The Sydney Women's Hospital in 1904 reported ten years' work, with nearly 4,000 cases, and not one death from puerperal sepsis. It is not a question of fine homes or comfortable surroundings. In outpatient practice, amidst the squalor of East London, the British Lying-in Hospital recorded nearly 30,000 cases with only three deaths from septic causes. Yet the cruel fact remains that, in England and Wales, there has been practically no lessening of the combined mortality from puerperal fever and accidents of childbirth during the last forty years. Almost the same might be said of Victoria. Medical practitioners vary in their grip of aseptic methods; but the lack of progress depends chiefly on the nursing. In England and Wales, less than half of the births are attended by a medical practitioner. It is not too much to claim that out of every ten deaths from puerperal sepsis, nine are probably preventable. A Midwives' Bill, somewhat on the lines of the British Act, is urgently needed in Australasia, so that proper training and registration may be compulsory.

The fall in the birth-rate has been deplorable. In England and Wales, between 1870 and 1905, the crude rate fell 23 per cent., and among mothers in childbearing ages the fall was 29 per cent. The highest rate was 36.3 in 1876, and the lowest 27.2 in 1905. The Registrar-General believes that 73 per cent. of the fall is mainly due to deliberate restriction. The birth-rate of London for 1906 was the lowest on record. Sidney Webb quotes a Friendly Society, composed practically of thrifty artisans, in which the birth-rate during the decade ending 1905 showed a fall three times as great as that seen generally during that decade in England and Wales. Speaking generally, it may be said that the fall is not noted so much in the highest or the lowest ranks of society, but is strongly marked in the middle classes. Australasian statistics reflect only too faithfully this widespread tendency, though since 1903 the tide appears to be slowly turning. In New South Wales and New Zealand, the fall extended over twenty years, but in Victoria the loss was concentrated into the ten years ending 1902. As far as this downward movement represents an increased recognition of responsibility on the part of parents, some extenuation may be pleaded; but so far as it implies unwillingness to accept responsibility, it can only receive unqualified condemnation. It is not merely a question of limitation. In the United States, it is estimated that there are 2,000,000 of childless homes. In Massachusetts one-sixth of the homes are childless, so that a century ago children were one-third of the population, whereas now they are less than one-fourth. The childless home lacks the deepest source of joy, and those who think they can cheat the purpose of marriage, without incurring risk of personal injury, simply deceive themselves.

In the saving of child-life, there is a far more cheering tale to be told. The mortality is greatest in the first year, and half of the mortality of the first year occurs within the first three months. The cities still show much more unfavorable results than the country districts, but the disparity is not so great as in former times. In Victoria, for example, the infantile mortality rate for the metropolitan district has fallen from 168.5 in the years 1873-80 to 112.6 in the years 1901-5, while in the extra-metropolitan districts the rates have fallen in the same time from 101.6 to 84.4. The

Australasian rates for 1901-5 vary from 125.9 for West Australia to 74.8 for New Zealand. Apart from the complex questions concerning the so-called "natural" infants, among whom the death-rate is at least two or three times as great as among others, and apart from the appalling results of inherited taint, the chief factor that decides the life or death of infants is food. Nothing can adequately replace the natural food, and to multitudes of mothers the spiritual reproach of Meredith might be literally applied—

"Look, woman in the West, there wilt thou see
An amber cradle near the sun's decline :
Within it, featured even in death divine,
Is lying a dead infant, slain by thee."

When the natural food is not available, no substitute can compare with cow's milk. But in great towns, how difficult the milk question becomes. In New York, milk is usually 48 hours old before it reaches the consumer, and during hot weather the resulting evil is intensified. In the poorer streets in New York, it has been estimated that even in midwinter the milk contains nearly 2,000,000 bacteria per cubic centimetre, whereas in a warm month the number rises to over 13,000,000. Even in fashionable streets, the corresponding numbers were over 300,000 and over 1,000,000. In bad streets, the numbers rise rapidly from morning to evening, a count at 9 a.m. perhaps giving 30,000,000 to 40,000,000, and at 7 p.m. 90,000,000 to 130,000,000. A federal text-book on the milk question has recently been prepared by a conference of experts in the United States, and it recommends a definite programme under which milk would be classified as "certified" or "inspected" or pasteurized. Certified milk would come from dairies periodically inspected, the milk being frequently analyzed, the cows tested by tuberculin, &c., and found free from communicable disease, the milk handled only by persons free from infective disease, the milk containing not more than 10,000 bacteria per cubic centimetre, and being not more than twelve hours old when delivered to the customer. Inspected milk would be somewhat lower in standard, but would be produced under similar conditions, from cows tested with tuberculin, the milk not containing more than 100,000 bacteria per cubic centimetre. No other milk would be sold raw. All inferior milk, or milk of unknown origin, would be pasteurized. The question of adopting this programme is now under serious consideration. In any scheme for the supply of uncooked milk, immediate refrigeration at the dairy is the prime necessity. Municipal milk depôts have been established in Liverpool, Glasgow, Bradford, and other places, but they are not a success financially. Kenwood voices a large body of opinion in preferring a system of "Infant Consultations," such as was inaugurated by the late Professor Budin, the directors recommending milk vendors who comply with their regulations. Excellent results have been attained in Philadelphia through the action of a Milk Commission of five medical men appointed by the Pediatric Society, a high standard of purity being fixed, certificates given to dairymen conforming to it, and all milk delivered in hermetically sealed bottles, with a slip certifying that the milk had been examined and found satisfactory within the last month. Pasteurization may be replaced by the Budde process of sterilising by peroxide of hydrogen at 50 degrees Centigrade, so that taste and flavour are unchanged, and the natural ferments are preserved or restored.

Public opinion is awakening very slowly to the importance of the early training of little children. The main features of character are fairly

defined before a child is three years old. Hence the levity with which many well-to-do parents leave their children during these years largely to the tender mercies of ignorant inexperienced servants is most deplorable. This is the time in which habits should be carefully formed, to constitute the basis of steady character. I would also appeal for the culture of the imagination of children, beginning with the simplest modes and proceeding to the highest. We are familiar with Mr. Gradgrind's philosophy of fact in "Hard Times." Edmond About, in his *Roman d'un Brave Homme*, speaks of the excellent education given to the hero by his father; but Positivism was already at work, and in the nursery of the next generation, the hero "pitilessly proscribed the most accredited legends, the most convenient hypotheses, the most official historic lies. The watch-word of the little class was to teach nothing to children that you cannot prove or explain." Poor baby positivists, is not this an over-true version of a widespread system, under which the imagination is stunted, the mental power shackled, the joy of life undeveloped.

We rejoice in the progressive legislation for infants and young children, especially for those most subject to neglect and cruelty; in the Acts for early notification of birth, for infant life protection, for children's courts, for inspection of schools and school children; and in all the forms of philanthropic work for the little ones that kind hearts have prompted. But how much remains to be done! How much permanent loss of sight might be prevented merely by disinfection of babies' eyes. The President of the American Medical Association, in his address last year, stated that unrestrained ophthalmia caused over 25 per cent. of blindness from disease; that over 30 per cent. of the school children of New York suffered from defective vision, and that in 75 per cent. their mentality was impaired by causes yielding to curative measures of a simple and permanent nature. Does this statement seem exaggerated, or would you confine its truth to children who have grown up under unfavorable conditions? The answer is not far to seek. Clement Dukes examined a 1,000 boys on their entrance to Rugby between the ages of 13 and 15. Here are some extracts from his report. Lateral curvature of the spine in 445 out of the 1,000: pigeon breast in 126; bowlegs, 64; knock-knees, 526; flat feet, 329; stammering, 70; hypermetropia, 40; myopia, 128; astigmatism, 27; colour blindness, 12; mouth-breathing, 112; chilblains, 437; varicocele, right 4, left 92; albuminuria, 157. He adds that in no instance were slight defects registered. On the whole, though the results of the inquiries already begun in Australia will doubtless be much more favorable, I cannot but conclude that we are only at the beginning of our task in the conservation of health during childhood and youth.

In adolescence and early adult life, enormous evil is wrought, especially in the upper classes, by a too restless excited life. Steady work, however hard, seldom injures health. Adequate sleep is a full corrective. But unsteadiness and restlessness, with irregular meals, irregular sleep, and continual excitement, induce the neurasthenia that is so common. People are worn out long before their time. Unfortunately it is precisely those of unstable neurotic type who are most liable to such worry of the nerves, and to the degeneracy that follows. There is, however, another form of neurasthenia, that Dr. Hellpach laid stress upon in his inaugural address to the Technical High School of Karlsruhe in 1907, the factory neurosis begotten by mechanical thought-killing monotony. Readers of Dickens will not need to be reminded of the remedial philosophy of Mr. Sleary

in "Hard Times." Let machinery and routine have full sway in all things, and, as Dickens says, the heart of infancy will wither up, the sturdiest physical manhood will be morally stark death, and the plainest national prosperity that figures can show will be the writing on the wall.

Concerning middle life, added experience serves but to emphasize the disastrous results of over-strain, whether mental or physical. Too strenuous a life reacts on the whole metabolism of the body, with inevitable auto-intoxication, gastro-intestinal, muscular, articular, cerebral. Hence come dyspepsia, muscle pain, gout, migraine; but more insidiously come all the troubles of kidneys, heart and arteries that are associated with high vascular tension. Excesses in flesh food, whether in red meat or in white, commonly attend the strenuous life, while Chittenden's researches have proved how little proteid food is necessary. Excesses in alcohol are almost as common, especially when brain and heart begin to weary with the constant strain. The moral is obvious, but obedience so often comes too late. Many books have been written with elaborate directions for the management of old age and the decline of life, but the secret of happy old age lies in sound family history, healthy joyous childhood, active temperate manhood and womanhood, recreation following work, imagination redeeming routine. These are the simple but essential antecedents that no special regimen, no quack nostrum can ever replace.

On questions of disease, only a very brief comment can be made. Bacteriological laboratories have not only reconstituted our notions of pathology, but have supplied us with new and potent weapons of defence. Antitoxin has robbed diphtheria of its terrors. Hydrophobia can be prevented even after the bite. Chantemesse claims that acute dysentery, if treated early, yields with certainty to his serum. Koch's discovery of the specific comma bacillus and of its life habits has brought cholera under comparatively easy control. In the relations between disease in man and disease in the lower animals, a whole world of new knowledge has arisen. The proved fact that the bacillus of plague comes to man through the rat and the rat flea has placed pestis major, for European communities, within the list of manageable diseases. Recognition of the fact that yellow fever like malaria is transmitted by the mosquito enabled the Americans in 90 days to convert Havana into practically a clean city. In the Panama Canal works in 1882, 1,900 men were employed, and the mortality was 112 per 1,000; whereas in 1905, under American control, 25,000 men were at work, with a mortality of less than 1 per 1,000; but no less than 4,000 men were engaged in exterminating mosquitoes. In 1907, the Chief Sanitary Officer told the graduating class of Cornell that there had been no yellow fever on the isthmus for over a year; that malarious fevers had been reduced to a minimum; that the special mosquito of yellow fever had been so reduced in numbers that yellow fever would not spread even if it were introduced; that among the 6,000 Americans connected with the Canal work, including 1,200 women and children, there was very little sickness of any kind, and the general appearance was fully as vigorous and robust as among a similar number of people in the United States. In Ismailia in 1897 there were 2,089 cases of malaria; in September, 1902, Professor Ross was placed in charge of operations; in 1905, there were only 37 cases, and nearly all of these were relapses. Here also I may remind you that Colonel Sir David Bruce, the hero of Malta fever, of tsetse-fly disease, and of sleeping sickness, was a native of Melbourne, born in Prahran.

The work of the health departments, civil and military, shows many records of glorious achievement. In rapid sequence upon improved dietary, Beri-beri has been banished from the Japanese Navy, and reduced to small proportions in the Army. In Oku's army Seaman tells us, there were less than 200 cases of enteric fever and less than 400 of dysentery, and only 40 deaths; whereas in the Boer War, certainly under worse seasonal conditions, Britain lost more than 13,000 men by preventible disease. In civil practice, the mortality from enteric fever in England and Wales has fallen from 384 per 1,000,000 in 1869, to 89 per 1,000,000 in 1905; and this is in addition to the immense decrease in the mortality from so-called simple continued fever, which was mainly enteric fever in disguise. Similarly in Victoria, the mortality from typhoid fever has fallen from 369 per 1,000,000 of the population in 1890-92 to 132 per 1,000,000 in 1906. In 1905 the rate was only 100. But defective drainage and water supply in the country districts still produce the inevitable results. Thus in 1906 the rate for the Metropolitan District of Sydney was 118, while that of the country districts of New South Wales was 212. If the mortality from typhoid fever throughout the Commonwealth were brought down to the level that obtains in England and Wales, 270 lives would have been saved in 1906 alone.

The great reduction in the death-rate from tubercle (the mortality from phthisis in England and Wales has fallen 50 per cent. within 40 years) must be attributed mainly to the steady influence of the hygienic measures that have been quietly and persistently pushed forward by the various Boards of Health. The work of the British Royal Commission on Tuberculosis has established the unity of the disease. We have learnt that bovine and human bacilli are different strains of one organism; the bovine more toxic, shorter, growing slowly on culture media, broth cultures tending to remain alkaline; the human less toxic, longer, growing more rapidly, broth cultures tending to become acid; these distinctions, with other minor points, serving fairly well as tests to determine the origin of any strain. The experts employed by the Commission reported that, out of 60 cases of human tuberculosis, fourteen belonged to the bovine type. Of 28 cases of alimentary origin, 13 were of bovine type. Among these 28, the glands of the neck were studied in nine, and three gave the bovine type. Of 19 with abdominal lesions, ten gave the bovine type. On the other hand, in three cases, bacilli of human type, after repeated passage through the calf and the guinea pig, took the bovine type. It was also discovered that, when human bacilli are introduced into calves by subcutaneous injection, the bacilli become widely diffused, and may persist for months in organs that appear healthy to the naked eye. These results are extremely serious, in view of such records as those of the Leipzig abattoirs, where of 9,303 cows slaughtered, 43.51 per cent. were tuberculous, or the estimate of the late Professor MacFadyean that in English cowhouses 30 per cent. were infected. Gilruth, reporting in New Zealand for the three years ending March, 1905, found over 10 per cent. of 45,742 cows tuberculous. Something however may be said on the other side. Findel of Breslau found that with guinea pigs, using Reichenbach's apparatus for respiration, if at least 62 bacilli were inhaled, tuberculosis followed in every case; but by the digestive way, a dose 6,000,000 times as large was necessary to be sure of producing tubercle in every case, and a dose 19,000 times as large always failed. It must also be remembered that, in England and Wales, pulmonary tuberculosis causes 69 per cent. of the total mortality from

tubercle. Hence, notwithstanding the experiments of Calmette and the theory of Behring, the frequency of the alimentary conveyance of tubercle may easily be exaggerated. Another aspect of the whole question is given by Newsholme's statement that the phthisis death-rate varies very closely with the total poverty-rate. Apart from poverty, Newsholme finds that the rates in the chief European countries vary chiefly with the degree of segregation of phthisical patients in general institutions. For the individual consumptive in the early stages, sanatorium treatment is full of hope. When the disease is far advanced, the main objective must be to prevent the spread of infection, either by adequate precautions in the home, or by segregation in suitable institutions. In Victoria, a notable step forward was taken at the end of 1907, when the most populous centres were declared by the Board of Public Health to be affected with the dangerous infectious or contagious disease, pulmonary tuberculosis, with the consequence that notification of all cases became compulsory. Enforcement of this notice is still contingent on the provision of adequate hospital accommodation for necessitous cases. The full development of these measures, coupled with more general appreciation of the value of fresh air and sunlight, should make possible at no distant date another reduction by one-half of the deadly toll levied by the Great White Plague.

Concerning the general progress of Medicine and Surgery, there is much cause for gratitude. Increasing knowledge of anæsthesia and continued developments of antiseptic and aseptic methods have made possible the addition of wide kingdoms to the domain of Surgery. Improved methods of diagnosis have made our knowledge of disease more precise, and have opened the way for more scientific treatment. New theories of chemical solution and of electrolytic dissociation have thrown great light on the action of drugs. A huge department of physical therapeutics has been created, and is already being threatened by the evils of excessive specialization. More attention is being paid to the adjustment of the individual, the care of the unfit, the neurotic, the insane. An enlightened press is doing much to quicken the public conscience in matters of public health. The standard of professional education is rising. The spirit of research is becoming more and more wide-spread. But so much remains undone or imperfect. We are still weighed down by ignorance and prejudice. When shall we be free from traces of the incubus that Wells so finely depicted in the *Story of a Simple Soul*:—"Above them, brooding over them, I tell you there is a monster. a lumpish monster, like some great clumsy griffin thing . . . like pride, like indolence, like all that is darkening and heavy and obstructive in life. It is matter and darkness, it is the anti-soul, it is the ruling power of this land. Stupidity. . . . But for that monster they might not be groping among false ideas to hurt one another so sorely; but for that, the glowing promise of childhood and youth might have had a happier fruition; thought might have awakened in them to meet the thought of the world, the quickening sunshine of literature pierced to the substance of their souls; their lives might not have been divorced, as now they are divorced, from the apprehension of beauty that we favored ones are given—the vision of the Grail that makes life fine for ever." Translate these burning words of the spirit into the humbler phrases of the body, and we have the aspiration that animates the medical profession, an aspiration that must surely realize itself step by step in actual fact.

In this mood I would bring my address to an end. The medical profession owes to the public a service of thought as well as a service of action. We shall not fail in the service of action. Let us see that we fail not in the service of thought. And now we go to our Sections, there to rejoice together over our successes, there to take counsel in our difficulties, there to hearten one another for all that lies before us ; and so back to our various spheres of duty, made stronger by our fellowship, humbly striving to have more adequate answer to the questions—what hast thou done, what hast thou thought?

VOTE OF THANKS TO THE PRESIDENT FOR HIS INAUGURAL ADDRESS.

COLONEL THOMSON (Brisbane) said, in proposing a vote of thanks:—The very highest honour the medical profession in Australasia can confer on one of its members is to elect him to the presidential chair of the triennial Congress. The profession honours the man! May not the man honour the profession? (Cheers.) I am certain you will agree with me that, in the services he has rendered to-night—in his address, learned, brilliant, graceful—Professor Allen has refunded, and with interest, the high compliment paid him—and, honour for honour—honours are easy. (Cheers.) Custom has imposed upon the President a tax—his address. All past Presidents have paid it—save one; he evaded it, and I have an immense respect for him on that account. The tax, like most taxes, may fall unequally. I once delivered a presidential address, and at the time I thought it a very good one (laughter); but now, when I look back upon it, through the fierce glares of nine long Queensland summers, I think my audience was awfully bored. (Laughter.) How different to-night. Had the tax been repealed we should have been deprived of this great attraction—this great charm—the intellectual feast of the Congress. (Cheers.) It would be absurd of me to attempt to review this great oration, even if I had the ability, which I do not claim. It would be unbecoming on my part, alike to the orator and to this vast appreciative audience. There is ample in the address to invite hours of study and reflection; there are lessons taught, to be long remembered; there are practical suggestions which should become part of the routine of our daily lives. (Cheers.) I have to congratulate the profession in Victoria on the success of its nomination; I have to congratulate the members of Congress at Adelaide, three years ago, on their unanimous acceptance of that nomination; I have to congratulate the profession throughout Australasia on its generous recognition of the Congress and its President by increased membership and personal attendances. I have to congratulate you, citizens of Melbourne, on your famous University and its Medical School—(cheers)—and on the men whose strenuous efforts gave these learned institutions the renown they now enjoy; and it is among these men—among the very greatest of them—I bracket our distinguished President. The time may come when the labours of this Congress are only a memory, a pleasant, a charming memory; but the work of Professor Allen, his magnificent museum, his devotion to his great school with all its objects and interests are matters of academic history, as imperishable as the University itself. One of my friends—one of the many charming hostesses who are already trying to demoralise us with their hospitality—said to me this evening, “If you want to be appreciated, don’t speak for more than three or four minutes.” (Laughter.) I intend to take her

advice. Your Excellency, may I request you to invite this vast audience to tender Professor Allen a very hearty vote of thanks for his inaugural address.

HIS EXCELLENCY then put the motion, which was carried with acclamation.

PROFESSOR ALLEN, in rising to respond, was received with loud and prolonged cheers. He said:—Your Excellency, Ladies and Gentlemen—I can only offer you my sincere acknowledgments for the very kindly way in which you have received my address. I know too well the defects in it, but I have tried to say what my heart prompted me to say. I have spoken in all honesty and sincerity of purpose, and I can only thank you once more for the very kindly way in which you have received my efforts. (Cheers.)

VOTE OF THANKS TO HIS EXCELLENCY THE GOVERNOR.

PROFESSOR ANDERSON STUART (Dean of the Faculty of Medicine of the Sydney University) rose to move a vote of thanks to His Excellency the Governor. He said:—Your Excellency, Ladies and Gentlemen—It is one of the privileges of old age to be reminiscent. That is my privilege now, because I cannot help remembering twenty years ago when the first meeting of this Congress in Melbourne was held in the Wilson Hall. At that time we sat under the presidency of the late Sir Thomas Fitzgerald, whose personality was the soul of that meeting. We have had some excellent meetings since that time, but we have certainly had none better. (Cheers.) I also think of my own coming to Australia, twenty-five years ago, and when landing in Melbourne I proceeded at once to the Medical School in Melbourne, which I take pleasure in remembering was the first Medical School to be established in the Commonwealth. (Cheers.) Colonel Thomson has referred to the distinguished connexion of Professor Allen with that Medical School. With all he has said I entirely agree; but I am also bold to believe that Professor Allen will agree with me when I also remember our old friend, Professor Halford, for he it was who laid the first stone of the Medical School here in Melbourne. I think, also, I might take the liberty of saying to the Lord Mayor of Melbourne, on whose recent honour we so cordially congratulate him, that we who come to Melbourne occasionally, at rare intervals of well nigh a generation, cannot help remarking the wonderful progress which this city is making in every desirable particular. Time was when Melbourne was not a beautiful city, but it is a beautiful city now, and it is becoming more beautiful every day. (Cheers.) The purpose of my rising was not to exceed the hostess' allowance of five minutes, but to say that we should ill do our duty here if we did not give a unanimous and hearty vote of thanks to Sir Thomas Carmichael for being with us this evening. (Loud cheers.) Even Sir Thomas brings back pleasant memories to the old, because his name in Edinburgh, from which I have come, was a household name to me; and when I received a message that he was coming to Victoria, I thought, "Well, Lord Hopetoun came from there, and now Sir Thomas is coming from there also." You cannot get away from Edinburgh anyhow. Even Colonel Thomson comes from Edinburgh. His brother was my chum when I was a student at the University. I did not know Dr. Thomson until I came here. I cannot get away from the Thomsons. We were all John Thomson's bairns somehow. (Laughter.) Now, ladies and gentlemen, I have come to the end of my tether

It is a short one, and I propose with great cordiality a hearty vote of thanks to Sir Thomas Gibson-Carmichael for being with us this evening. (Loud cheers.)

The vote of thanks was accorded with acclamation.

THE PRESIDENT said:—Your Excellency, on behalf of the Congress and of this meeting, I have now to offer you our warmest thanks for your great kindness in being here and declaring this Congress open. I wish to add that we are also very grateful to Lady Gibson-Carmichael for her gracious presence with us this evening. (Loud cheers.)

HIS EXCELLENCY said:—Ladies and Gentlemen, I think all that I need say is "Thank you." (Cheers.)

THE PRESIDENT then declared the Congress adjourned till the following morning at 10.15, in the Wilson Hall.

GENERAL MEETING OF CONGRESS.

TUESDAY, 20TH OCTOBER, 10.15 A.M.

(WILSON HALL).

On the re-assembling of the Congress, 200 members being present, the Address on Medicine was delivered by G. E. Rennie, B.A. Syd., M.D. Lond., F.R.C.P. Lond., Physician to the Royal Prince Alfred Hospital, Sydney, President of the Section of Medicine, who took for his subject, "The Mental Factor in Medical Practice." The address is printed at the head of the Proceedings of the Section of Medicine.

THE PRESIDENT (PROFESSOR ALLEN) expressed the thanks of the Congress to Dr. Rennie for his valuable address on a most important and most difficult branch of medical science. It would have been a great misfortune if the subject had not received the attention which it deserved. (Cheers.)

THE PRESIDENT announced that all Masonic members of Congress had been made members of the Masonic Club; that all members of Congress had been made members of the University Club; that the Oakleigh Golf Club desired to entertain members; that the Committee of the Women's Hospital had directed that the institution should be open to the inspection of members at all times. He then declared the Congress adjourned till Wednesday, 21st October, at 10.15 a.m.

GENERAL MEETING OF CONGRESS.

WEDNESDAY, 21ST OCTOBER, 10.15 A.M. .

(WILSON HALL).

Congress re-assembled in the Wilson Hall on Wednesday morning at 10.15, the President (Professor Allen) being in the chair. There was a very large attendance of members.

THE PRESIDENT said:—We have met this morning to discuss certain proposals concerning the relations of the medical profession to public hospitals. Certain resolutions, already printed and circulated, have been prepared by a special committee consisting of Dr. W. Moore (convener), Dr. A. V. M. Anderson, Mr. G. Cuscaden, Mr. A. Lewers, Mr. W. J. Long (Bendigo), Dr. D. Murray Morton, Mr. F. J. Newman (Geelong), Dr. R. Scott (Ballarat), Mr. J. Ramsay Webb, and Mr. T. A. Wilson (Creswick). The text of the resolutions is as follows:—

RELATIONS OF THE MEDICAL PROFESSION TO PUBLIC HOSPITALS.

1. To be proposed by Dr. Worrall, Sydney, seconded by Dr. H. Swift, Adelaide:—

That in the opinion of this Congress no payment whatever should be taken from patients treated at hospitals maintained by the donations of the charitable and by Government grants.

2. To be proposed by Dr. Butler, Hobart, seconded by Dr. W. St. J. Dansey, Sydney:—

That, except in cases of accident or emergency, every patient should, before being treated, be required to sign a declaration of inability to pay for medical attendance; and that as far as possible all patients seeking treatment should bring a recommendation from their own medical attendant, whether lodge or other.

3. To be proposed by Dr. John Thomson, Brisbane, seconded by Dr. W. Anstey Giles, Adelaide:—

That this Congress affirms the foregoing resolutions as the foundation of any real reform in the present abuse of hospitals, and is of opinion that other hospital accommodation would extend naturally to the inclusion of all those able to pay for medical treatment.

4. To be proposed by Dr. W. Chisholm, Sydney, seconded by Dr. Cuscaden, Melbourne:—

That this Congress is of opinion that it would be a great gain to the community if an executive body were established to—

- (a) Administer the Government grant.
- (b) Classify hospitals.
- (c) Prevent overlapping of hospital work.
- (d) Insist on inquiry for the prevention of imposition.

- (e) Insist on the adoption of uniform methods of keeping accounts.
- (f) Close unnecessary hospitals.
- (g) Prevent the establishment of unnecessary hospitals.
- (h) Foster economy in hospital administration.
- (i) Promote the transfer of patients from one class of hospital to another.

THE PRESIDENT requested Dr. W. Moore, as convener of the special committee, to shortly place the whole subject before the meeting. (Applause.)

DR. W. MOORE (Melbourne) said :—Mr. President, Ladies and Gentlemen—As convener of this committee I find myself in a more prominent position than I care to occupy, and I will occupy it as shortly as I may. It is to be hoped that members of the Congress realise the great importance of the discussion that is about to take place. Apart from its own intrinsic importance, it probably means the taking up of a new branch of work by this great body. The holding of a discussion on the relations of the medical profession to public hospitals is quite a new departure at our Congress meetings. The resort to charitable institutions of patients from an ever-widening circle is rendering the consideration of this matter by the medical profession an absolute necessity. The constantly increasing expenditure on the maintenance of hospitals makes this question a very important one to the general public, and even to our legislators, since the greater part of the funds of these institutions comes from the public Treasury. It would be easy to show that the whole community is interested in the just and right settlement of this important question. It will no doubt be said by many outside the profession that we are considering this question purely from our own and purely from a selfish point of view. Now, whilst it will be clear from what is said to-day, and whilst it may as well be distinctly understood from the outset that we are looking after our own interests, we may fairly claim that the medical profession has never been known to neglect the interests of the general community, still less of the sick, and least of all of the sick poor, and to-day it is not going to depart from its universal and well-known practice. Further, as in all matters of social or economic politics I hold most strongly that there is always a harmony, that what is really right and beneficial for one cannot be wrong and harmful to another, so in the matter that we are about to consider I am satisfied that what is most for our real advantage will be found to be most to the advantage of the sick poor and of the public generally. In other parts of the world, in Austria, Switzerland, France, and other parts of Europe, this same question has forced itself upon the attention of the medical profession. In Great Britain, for the last few years the British Medical Association has had a special committee considering it, and this committee has had one or two conferences with the managers of hospitals in Great Britain. Though the matter has been under consideration for some time, very little has yet been actually done. But the committee is educating the profession, it is formulating certain definite clear-cut opinions, and is bringing them before the general body of the British Medical Association, and through the British Medical Journal before the profession throughout the English-speaking world. The committee appointed by the executive of the Congress to deal with this matter, recognising how great the subject was, how many and how difficult are the questions that may demand consideration in different parts of so vast an area as Australasia and New

Zealand, and knowing how slowly the British Medical Association had been obliged to proceed, decided to select certain fundamental principles in regard to hospital management and bring them before you. We hoped that in regard to them practical unanimity might be attained. Even if we attain to that very enviable result it may still seem to some that we have done nothing. But the committee did not hold that opinion. It considered that we would then have a basis on which each State, each capital city, each district, even each hospital might be expected to work out its own salvation. It must be borne in mind that those are ideal principles to which we hope the Congress will give its approval.

To us it seemed that the first two resolutions are absolutely essential to any reform in the present abuse of hospitals. The third resolution is introduced to allow members to point out the various ways in which the situation is to be met that would be created by the exclusion from public hospitals of those who are not well off, but who without great hardship can pay for medical treatment. The fourth resolution is a general one which we will probably all be able to accept with very little discussion.

At the recent meeting of the Association held at Sheffield, one of the resolutions proposed in the discussion on hospital management was "That consultations shall, as far as possible, be encouraged in out-patients departments." There was a long discussion on the resolution. An amendment to the effect "That out-patients departments should be abolished except for the purpose of consultations" was proposed, but was negatived. Finally an amendment "That the out-patient department should be principally a department for consultations," proposed by Dr. Alfred Cox and seconded by Sir Victor Horsley, was carried. But the committee that drafted the resolutions that are to be discussed felt that the time was far from ripe for such action on our part. There are other important questions involved in it, such as the provision of provident dispensaries. But we think this is a matter that should be thought about deliberately, as action will doubtless have to be taken some time. Another great and burning question is that of intermediate hospitals. Under resolution 3 an opportunity is given to discuss this question, but even here the committee felt that the time had not come for a definite pronouncement on the subject. Speaking for myself, not for the committee, I think it is a very dangerous question for us to play with. It surely is scarcely for us to be advocating the provision of more hospitals. (Hear, hear.) What we will have to do is to see that, if such hospitals are started, they are managed under conditions of which the medical profession can approve. (Hear, hear.) Another matter that we only touch in our fourth resolution is that of inquiring into the suitability of patients. The British Medical Association carried a resolution at Sheffield "that some means of investigation into the circumstances of the applicants for relief, by means of an almoner or other agent, shall be employed in all medical charities." This will become a very important matter when we secure the abolition of the taking of payment from patients. When payment is taken, and investigation is conducted by a hospital official, as is almost invariably the case now, the inquiry is simply a farce. (Hear, hear.) Another important and very difficult matter that has not escaped the attention of the committee is the special difficulties in dealing with small country hospitals. Here again we would remind members that we are proposing ideal principles that we wish to see attained as nearly as possible. In working out details later on, it may in some cases be necessary to depart slightly

from our ideals. It has seemed to me that, in working out the principles on which the small country hospitals should be managed, we might well follow the example of the British Medical Association in regard to cottage hospitals. Of course, we recognise the great difficulty that there will be in carrying out the reforms that we see to be necessary. Still, if in certain important hospitals we can abolish all payments, and secure the more general requirement of letters from the ordinary medical attendants, then with some system of strict inquiry into the condition of patients, abuse would readily be reduced to a minimum. With the system at present in vogue in most of the States, the abuse of hospitals is simply invited. The carrying of these resolutions will in any case be but a beginning in this matter. I think, as representing the committee, I may suggest that too much be not made of the difficulties that will be met with in obtaining the reforms that we desire. Of course, there will be difficulties; nothing worth having can be got without surmounting difficulties; but I am satisfied that there are no difficulties here that a united profession could not easily surmount. (Hear, hear.) Certain it is, too, that many other urgently-needed reforms in regard to public hospitals could be easily obtained if the principles laid down in the resolutions to be brought before you were generally recognised in the management of hospitals. The one important difficulty to be overcome is that of finance. Now, I think that if we were to exclude those who can afford to pay, and let it be known that hospitals are only for the poor when sick, and that the poor are never excluded, then we might fairly expect that the charitable public would contribute more largely to the hospitals. (Hear, hear.) On the other hand, this difficulty of hospital finance might also be partly met by economies in management, and in this much might be done by the medical staffs. But this difficulty of finance, the only real difficulty in our way, should in this discussion be disregarded. We are advocating ideal principles, the difficulties are matters to be dealt with later on. (Applause.)

DR. WORRALL (Sydney).—Mr. President, Ladies and Gentlemen—The resolution which has been entrusted to me is “That in the opinion of this Congress no payment whatever should be taken from patients treated at hospitals maintained by the donations of the charitable and by Government grants.” The first question in connexion with the matter is, “Is there any cause for alarm?” Beyond all question. I call as witnesses every man here in a large practice (hear, hear), or connected with a large general hospital. I could give innumerable instances from my own experiences if it were necessary, but it is not. The abuse has reached such great dimensions that it has aroused public attention, and if you will allow me I will read you one or two paragraphs from a weighty leading article in the *Argus* of last Saturday:—“Consequently we have on the part of the public an ever-increasing complaint of the cost of medical treatment, and on the part of the profession an ever-growing protest that its purely voluntary service is being more and more imposed upon by the increased demands of public hospitals. While specialism in medicine and surgery is making for greater accuracy in diagnosis and treatment, it seems hardly reasonable that it should curtail the bread and butter of those who seek thereby to make their art more perfect. . . . What is to be the solution? The present drift appears without doubt to be in the direction of State-paid medical service. (Hear, hear.) The question that both the public and the profession have to face is whether this present tendency is, or is not, merely a drift. The State-paid

medical practitioner seems scarcely possible as a practical realization. Mallock's factor of ability appears certain to upset any idea that individuals will give their services to the State for a salary which they could double or quadruple on their own initiative. . . . And it is more than doubtful if the public would find any real comfort in the ministrations of a State-paid medical service. (Hear, hear.) The appraisal of personal skill is, after all, a matter of opinion, and is only one factor in the selection of a medical attendant. It is the personal relation between doctor and patient that gives the real value to medical advice, and such a relation must to a great extent be warped where the sense of personal obligation is stifled in both directions, as it must be if the doctor is to become a servant of the State. If the present tendency is a drift, then it is high time the drift should be stopped. And it can only be stopped by an effort from both sides. The public must decline to allow charitable institutions to be made use of by those who are not entitled to their accommodation (hear, hear), and the profession must bring itself to countenance a more elastic scale of fees, that will provide a fair remuneration for every division of labour, and permit the patient to feel that specialization in medicine and surgery is not going to spell ruin. The interests of profession and public are mutual and not antagonistic; and if this were kept in view on both sides, there would be less difficulty in adjusting present differences on a basis of mutual advantage. The fact that medicine has lost most of the atmosphere of aloofness and mystery with which the older physician surrounded himself, and that modern doctors are anxious to meet each other scientifically and socially in an assembly like the present Congress, is evidence that the laws of the profession are not unadaptable to altered conditions."

I think I might say no more as to the extent of the abuse. Let us turn to the cause and remedy. The cause lies in the fact that the people become demoralized by the multiplicity of hospitals and charities, and by the manner in which these hospitals compete for clients and support. These hospitals have in many instances been established by well-meaning people who are not happy unless running some charity, or by individuals or corporations who use those means of increasing their incomes and extending their influence. In consequence, the people have gradually lost that sense of shame that they at one time felt at accepting hospital relief while having means to pay for it. (Hear, hear.) "Every one does it; why not ourselves?" That's the cry in connexion with this matter. Let me read what Chief Justice Madden is reported to have said at the annual meeting of the Austin Hospital: "The people of the State annually give £300,000. Of this, £150,000 is expended on the patients and the other £150,000 melts away in the management expenses of the innumerable 'institutions,' 'refuges,' 'homes,' and what-not, that hang their signs out in almost every street." Perhaps you will bear with me while I read from the *Sydney Morning Herald*. After touching on the fact that there are four Women's Hospitals in Sydney within a mile of each other, and asking why it is believed these various institutions are considered necessary, it goes on to say:—"The public are daily called upon to subscribe to all sorts of charities, and in almost every instance one finds that there are two or more quite independent bodies with precisely the same objects. There would be a great saving of public funds, and much more efficient and economical administration, if the Government refused a subsidy to all new charities which were already adequately provided for, and insisted on the amalgamation of those already in existence, as far as seemed

advantageous to the community." (Applause.) It is certain that the present system of rival charities is having a disastrous effect upon the morale of the people. Each charity can show how largely it is patronized, and this is put forward as proof that "it meets a long-felt want." In reality the increasing numbers who attend the hospitals and other charities indicate that the self-help which has hitherto led the working classes to join benefit societies and to make other thrifty provisions against a "rainy day" is being rapidly undermined, and that, therefore, the whole question calls for the grave consideration of the Government. Now, this brings me to point out that in this matter of hospital abuse the interests of the friendly societies and the medical profession are identical. (Hear, hear.) The difficulty that the friendly societies at the present time find in maintaining their membership and filling their ranks is largely due to the competition of the hospitals. (Hear, hear.) Only a little while ago my gardener had his family ill. I said to him, "Are you in a lodge? What doctor is attending you?" He said, "I have never joined a lodge; we always go to the hospital, and if we are too ill the City Mission sends its officer." And then he said he had an invalid pension. "When I am invalided I get an invalid pension, and when I am old I will get an old-age pension." You see what bad effect is produced by this unwise charity and foolish legislation. Every student of history must admit that the loss of self-reliance in individuals composing a nation has been a very important factor in its decline and fall. (Hear, hear.) When the Carthaginians came to rely on mercenary armies to defend them, their doom was sealed, and when the Roman populace sunk so low that their people were fed in the market place daily, they lost their thrift and self-reliance, and fell an easy prey to the hordes of barbarians that swept over Europe, the units of which had been trained to rely on themselves, each for each and all together. By the way, that is what I would like to see animating the medical profession, each for each and each for all, and not that individual action which strikes at the root of common good and general interest. (Cheers.) The loss of self-reliance, according to Gibbon, was an important factor in the downfall of the Roman Empire, and unless our politicians take a common-sense view of the effect of this piece-meal socialistic legislation they bring in from time to time, calamity will come to us. There should be no pensions unless they be based on contributions made during a man's active work, as in the German Government insurance system. Thrift and self-help must be fostered, not destroyed.

DR. DOYLE (Newcastle).—I rise to a point of order. Dr. Worrall is starting out on a very wide field of discussion. I wish to know, if, when he has finished, it will be competent for another member to start out and show the fallacy of his anti-socialistic speech.

THE PRESIDENT.—I cannot answer any general question. I can only deal with a matter when it arises. Dr. Worrall has been dealing with self-reliance in communities. I think in that he is within the realm of this debate. (Hear, hear.) I will warn Dr. Worrall, however, that his ten minutes is very nearly up.

DR. WORRALL.—I bring this forward not because I want to attack socialist legislation, but to show that the trouble lies much deeper than many are disposed to think. What steps should be taken to remedy the evil? In my opinion the foundation of reform lies in the resolution which has been intrusted to me. Hospitals must be stamped beyond all question as charitable institutions. The payment to the secretary of

the hospital of a sum of money for which a receipt is given leads many to believe that this is the full value of the services which they receive; others use it as a salve to their consciences, and many more hug themselves for having obtained by a little chicanery what would have cost them more outside. Let me give an instance of how payment to the hospital reacts upon the patient and robs the surgeon. Within the last few weeks I operated upon the wife of a leading citizen in the Sydney Hospital without knowing who she was. Some few days afterwards, in going through the hospital, she told me who she was. I said, "If any one told me your husband had sent you in here to accept charity I would not have believed it." She said, "I am not receiving charity, I am paying three guineas per week." If the hospitals will carry this resolution into effect, they will receive, as Dr. Moore has said, a large measure of support, and the strain on their resources will be less, because many who now use them will be diverted into private hospitals. I therefore earnestly commend this resolution to your favorable consideration, in the belief that it will enormously strengthen the hands of those who in the different States are doing their best to protect the profession from imposition and the national character from deterioration. (Cheers.)

DR. SWIFT (Adelaide).—Mr. President, Ladies and Gentlemen—I have very great pleasure in seconding this resolution so ably proposed, and I am fully in accord with all that has fallen from the lips of Dr. Worrall. It simply remains for me to emphasize some points he has raised. There are two main arguments in favour of the adoption of this resolution. One is that it will prevent the exclusion of the deserving poor (cheers), and, secondly, it will be the most efficient and really the only means of effectually checking the widespread and even increasing abuse of the charitable institutions by those who are well able to pay for treatment elsewhere. The wards of a general hospital are generally full, and if the actual state of the finances of each occupant of a bed could be carefully and conscientiously obtained, I am positive it would be found that a large proportion of those patients are ineligible to receive charity, and are depriving those not so comfortably off of their proper rights. Inability to obtain adequate treatment outside should be the sole consideration for the admission of patients applying for relief. Any patient who could afford to pay over £1 per week is not eligible to receive charity. If they cannot afford to pay £1 per week they should not be asked to contribute (hear, hear), for no matter how small the amount is, the patient is under the impression, generally erroneously, that he is paying for the services he receives. He says he is not receiving charity, and supposes that he is paying not only for his board and lodging, but also for his nursing and medicine, and also that the medical attendants are all paid by the board of management. His pitiful five shillings he considers a great deal, I suppose, but it is absolutely absurd. Further, it is a well-known dictum that "once a hospital patient always a hospital patient," no matter how much their financial position improves, and their neighbours follow their example. Mrs. A sees that Mrs. B, who is better off than she is, goes to a hospital to get relief, and she immediately says she will do the same. It would be wise to abolish all forms of admission orders. The poverty and sickness of the patient applying should be the sole consideration for admission. Every applicant for admission to a hospital should be compelled to sign a form stating his or her financial position, and inability to pay for adequate relief outside.

THE PRESIDENT.—That comes in under the second resolution.

DR. SWIFT.—They should be made to understand it, and should be dismissed from the hospital, called upon to pay for the benefits received, and be prosecuted for having obtained relief under false pretences if it is found that their statements are untrue. I have been connected with the administration of hospitals for over twenty years, and am fully conversant with all difficulties in preventing the admission of ineligible people for charity, and am more and more convinced that the only really efficient way is to take no payment at all. If no fees were demanded, and if every care were taken by the clerk to exclude all patients who can pay, and if every applicant had to sign a form on the lines mentioned, the number of ineligible patients seeking admission would be reduced, the deserving poor would be more promptly attended to, and the charitable public would not be robbed by unscrupulous people. (Hear, hear.)

DR. JOHN THOMSON (Brisbane).—You have all conceded that hospital abuse takes place. I might be allowed to quote from Dr. Fowler's address in medicine at Sheffield:—"But how will the practitioner fare for a livelihood in the future, if ever-increasing tendency of the people to expect gratuitous medical attendance continues unchecked, and neither political party considers it to be his duty to encourage habits of thrift among the working population? For thirty years, to my knowledge, the abuse of hospitals has been a burning question, and many earnest men have given time and thought to its consideration; yet nothing of any moment has been done. This abuse of the generosity of the profession will, in my belief, continue and increase so long as it is to the interest of the managers of hospitals to show that they are doing what it is the fashion to call 'a great work.' Having enjoyed a somewhat extended and varied experience of hospital committees, I have learnt to admire the devotion of their members to a self-imposed duty of ever-increasing difficulty, but this does not dispose me to blindly trust to their wisdom. The moment that the relief of mere numbers ceases to be a merit, and it becomes to their interest to show that they have done everything possible to insure that those who receive gratuitous medical treatment are really deserving of it, a great falling-off in the attendances will immediately occur." Then the question, perhaps, crops up, What are the public general hospitals to effect? Shortly, the treatment of the sick and suffering poor only. Technically you might put it, to provide for the immediate, and, if necessary, prolonged care and comfort of those requiring medical and surgical assistance who are unable to pay for it, and render first aid or undertake the emergency treatment only among all classes. No public hospital would shut its gates to any one who could well afford to pay, whose leg was broken and who could not be carried on. I remember one case in Brisbane of such a nature, and there was a tremendous outcry. Surely a man should get what attention his accident demanded. (Hear, hear.) There should be no payment of any kind. (Hear, hear.) There should be no scale of charges. Nothing should be received for services rendered. (Hear, hear.) All the moneys collected are for purely benevolent or charitable purposes, and it is for those purposes only that they should be expended. And because they are not so expended, many individuals use that as a reason for withholding their subscriptions, and there can be little doubt that the very prevalent idea that the hospital is a medical boarding-house and not a medical charity, is responsible for the lack of voluntary subscriptions. This would be my ideal, and many agree with me, but both I and they recognise the difficulties to be overcome before it could be obtained. In bush and mining towns it would be well-nigh

impossible; there are many in both places who, while actually homeless, are very far from being penniless, and are compelled to seek and willing to pay for the aid which can only be obtained at the district hospital. In larger centres, some departments—Police, Defence, Railway, &c.—and some employers, shipmasters and others, have to find medical attendance for their employés, and they do this by arranging with the hospital to pay a daily stated sum, in the case of Brisbane, 4s. 3d. That would have to be met and arranged. Then, unfortunately, there are the mistakes committed by our own brothers. (Hear, hear.) There is the patient requiring surgical assistance, not necessarily poor, but unable to pay the cost of an intricate operation, or the fees which a successful surgeon is entitled to expect. These surgical fees give rise to a good deal of professional misunderstanding and hospital irregularity. The surgeon is consulted, an operation is advised; the question of cost arises; fifty guineas are mentioned; shake of the head follows; and then comes the verdict—"My fee is fifty guineas, and if you cannot afford it you had better go to the hospital (a public one) and I will operate there." *And he docs.* (Hear, hear.) He might have suggested that a junior brother would willingly and successfully undertake the case for one-third of the amount. But why should he? The chances are that the patient would prefer the man of his choice, would sink his pride, save his purse, and boast that he was a full paying patient, relieved of all legal and moral indebtedness, because the institution charged him £1 or 30s. per week. And lastly, though far from being lastly, is a dishonest man who can well afford to pay "outside" charges, he lies about his money, he loafes in a hospital, and he robs the benevolent who support it. I am glad to see that this resolution does not carry with it any recommendation of what is to be done. It only insists on the fact that no payment should be accepted, and I thought of touching on what appeared to me to be a remedy; but I fancy my time is up, and I can only support the motion with great pleasure indeed, and some considerable knowledge. The last speaker said he was associated with public charitable institutions for over twenty years; well, I go ten better, for I have had over thirty years' connexion with them. I have met during that time with many of the irregularities that we are endeavouring to correct. (Applause.)

DR. MASON (New Zealand).—I have no wish to offer any opposition to this proposition, but I want to point out some of the difficulties which would obtain at least in New Zealand. The wording of the resolution does not in any sense define what a hospital is. There are a large number of people who suffer from ailments which require other treatment than purely surgical treatment, and it seems to me that the previous speakers have had a surgical hospital in their mind's eye. (No, no.) There is no definition of a hospital in the resolution. What are we to do with cases of scarlet fever and the more pertinent matter of consumption? Take a country such as ours, where we have only one private sanatorium. The State has a highly specialized institution costing a great deal of money, and why should a man who is absolutely unable to pay anything at all be able to avail himself of this highly specialized institution, while I or any one else able and willing to pay a little towards our keep should be kept out? That is one of the problems which this resolution offers. If the definition of the word "hospital" was altered, this would disappear. It has been held in New Zealand that any one who pays rates is entitled to go into a hospital. (Hear, hear.) But whether that is right or wrong, it does not do for a civil servant to say.

DR. J. W. BARRETT (Melbourne).—Like Dr. Mason, I do not wish to seem to oppose a very sincere and honest attempt to effect a reform very urgently needed. But I do desire to indicate a number of the difficulties which would be in the way of carrying such a drastic resolution as this into effect. In the first instance, I would like to draw your attention to something I distributed myself this morning, conveying the facts and showing the tendency of the hospital movement in Victoria, and it seems from the figures supplied by the Charities Inspector that the time is not very far distant when we shall have an attempt made to put the hospitals of Victoria on the same footing as State schools, to which every one may obtain access, officered by medical men and with a very considerable limitation of private practice. That objective is in sight, and is more voiced than you are aware of. About the abuse and the lines of the reform to be conducted there can be no difference whatever. Under the existing Victorian law, any one can obtain access to a hospital in Victoria. The law is so made that there is no limitation, and there is no definition of the person who may. The only restriction so far as I know that is placed on admission is that the hospital authorities have disregarded the law in some cases, and have excluded patients whom they find to be well-to-do, or they have compelled them to sign a declaration of inability to pay; but as no prosecution has taken place because any one has committed perjury, the demanding of a declaration has not got over the difficulty. The total sum expended on hospitals in Victoria is £148,055 a year, and on combined hospitals and benevolent asylums £23,618, or a total of £171,000. The total sum contributed by patients is £27,000, which is somewhere about 15 per cent. of the total amount. If this resolution were carried into effect and you stopped the whole of those payments, you would close up in all probability certain hospitals and certain portions of hospitals. The question which the medical profession and the business men who conduct the institution would be faced with would be, If you take away this £7,000, by what means do you propose carrying on? I do not say that is an insuperable difficulty. I have before me the report of the Inspector of Charities, who says, "Patients' contributions and fees this year realized £26,852." Objection is frequently expressed to this source of revenue. No one, however, is required to contribute who can sign a declaration of inability to pay, and those who contribute have to declare that they are unable to pay the ordinary medical and nursing fees. The average stay of a patient in a hospital is about four weeks, and in very few cases is the amount contributed more than £1 per week. Now, if independent inquiry were made into the circumstances of patients, and only those were admitted to hospitals who were absolutely unable to pay fees outside—if it was limited to that—why should not those who were justly inmates of a hospital contribute according to their capacity, and why should they not be self-respecting members of society instead of being made paupers, whether they wish it or not? Would it not be better to amend this resolution, inserting the words "unless inquiry is made into the circumstances of the applicant by some independent body." In that case, fees would only be taken in the event of the profession being thoroughly satisfied that it was a suitable subject for hospital treatment.

DR. W. R. BOYD.—That is the present custom.

DR. BARRETT.—There is no independent inquiry. It is by the hospital staff, and I agree with the previous speaker in regarding absence of independent inquiry as a weakness that is an essential feature of the

position. No reform in hospital management is worth anything unless it is accompanied by a constructive movement. Unless you have means of dealing with people of moderate incomes, say £300 or £400 per year; unless you can distinguish between those requiring out-patient and in-patient treatment; the friendly societies can provide for out-patients, but a man with £150 a year, and a family, who is afflicted with pneumonia cannot be treated in his own home, and will go to a hospital unless we can provide another place in which he can be treated in a self-respecting way. It is for the profession to take action and organize institutions to meet those requirements. In that circular you will notice I have indicated the necessity for the medical profession organizing and controlling such institutions. Left in the hands of any one else, that would be an abuse. If they are left to public philanthropy, no regulation will be observed. The figures show the extent to which the drift is occurring, and I again ask the members if they will seriously turn over in their minds the desirability, instead of simply passing a resolution which I think will have very little effect, of making a forward move, and themselves organizing these institutions, and meeting the inevitable objection which will be raised by the public by saying we were providing a reasonable way by which the great mass of the people can get attention in accordance with their means. Unless that is done, the sweeping of people into hospitals will go on. I don't want to disturb the harmony of the meeting or reflect on the splendid work done by the committee, but I will be grateful if they will accept the suggestion and insert in the resolution the words. "unless some inquiry has been made into the circumstances of the applicant by some independent body."

DR. ROBERTON (Auckland).—Several of the previous speakers have hoped for unanimity, and I think we have it in part, and that is in so far as the existence of the evil goes. While the profession in almost every part of the world has to fight the same evil in connexion with the abuse of hospitals, the special difficulties which have to be met with in different parts of the Empire require different treatment. The exigencies of local conditions may render measures, which in one centre are the most effective, in another a positive danger. We all hope that the object of the movers of the resolutions before the meeting may be attained, and their hands strengthened by an acknowledgment that they are best fitted to find the remedy most suitable for their own States. My chief object in speaking is, however, to emphasize the special conditions obtaining in New Zealand, which render the acceptance of that resolution in an unmodified form impossible to those of us who come from New Zealand. We have had experience of organized control of hospitals by the State now extending over more than twenty years. State control practically effects the application to New Zealand of the principles embodied in several of the resolutions we are at present discussing. I think that, under heading No. 4, there is not one point that State control is not grappling with, putting in the hands of responsible local public bodies the administration of funds and other duties which must be done locally. The system is not perfect, far from it, but the able administration of the late Dr. Duncan McGregor and the active work of the present Inspector-General, acting in sympathy with the medical profession, has effected much and promises more. The condition now is that all public hospitals are supported and controlled by the State. Voluntary public hospitals do not exist, and voluntary support is small, one-thirteenth of the total, and diminishing. Indeed, one-fifteenth does not fairly represent the real amount, for much included

is not properly voluntary. It is strange that the Act meant that hospitals were to be supported by voluntary contributions, but it does not effect that. What was proposed by the resolution was considered in New Zealand years ago by Dr. McGregor and by various Governments, and the principle has been shown in actual working time after time locally. Of course, we have no large centres as here. Our largest centre, Auckland, has barely 100,000 people, and our largest hospital has from 220 to 240 beds. It was condemned by Dr. McGregor and the late Mr. Seddon, as Dr. McGregor put it, "because it was a direct incentive to pauperism, itself a system of pauperization." The chief defect of the New Zealand system is that the Act of 1884 (amended in 1885), under which hospitals and charities are administered, omits any special enactment that the public hospitals of the colony are for the poor. By implication the Act is specially for the poor, but a decision from the Bench and legal opinion generally is that the hospitals of the colony are open to all who are in need of medical or surgical treatment in hospitals. Remember *that*, any of you who are urging for legislation in your State for the control of hospitals. See that there is some provision that the hospitals are for the poor. The one saving clause of the Act in this respect is one containing five small words, which states that every patient must pay according to his means, and, if rightly used by the local Hospital Boards, this clause would be effective in preventing abuse by the well-to-do. Some Boards do enforce it, while others do not. Some deliberately ignore it, to pander for local popularity or for political purposes. Now, if this clause were abolished in New Zealand, as without doubt the resolution before us asks, the only safeguard of our profession in New Zealand would be removed.

Throughout New Zealand we have two great dangers in this direction; first a strong feeling, due to ignorance, that because hospitals are supported by public funds (that is, by local rates and from the Consolidated Revenue in fairly equal parts), all, especially all those sufficiently well-to-do to pay rates, should have free hospital treatment. Many consider they pay for such treatment in paying their rates. They would let the poor go to the wall. There is another danger throughout New Zealand. There is a strong undercurrent of socialists, who would put on the State the whole function of providing medical and surgical treatment for the people, poor or rich, at the expense of the ratepayer. The difficulty is not in dealing with the very poor or the well-to-do, but with the intermediate class, who can afford some payment, but not full payment. At present they are not satisfactorily provided for. Adequate prevention of the evil seems to be possible only by a graduated scale of charges according to the income of the patient, and inversely according to the number dependent on the person concerned. The adoption of No. 1 by any community is impossible without provision for those able to pay something, but not full charges. The more logical way of proceeding is, to my mind, to first establish these part-payment hospitals, perhaps with paid staffs, and then carry out the principle of no payment in State hospitals for those who deserve or require gratuitous treatment. But that will not do away with imposition, and is, after all, the creation of additional hospitals with additional and unjustified expense, at any rate in small communities when the two classes can be treated together. The adoption without modification of the resolution will lead to three unfortunate results. First of all it will emphasize a class distinction of a kind which is against the spirit of the times. Secondly, if it is passed without

any provision for the intermediate class, it will exact from the sick poor payment which involves hardship and is against the spirit of the liberal profession to which we belong; and thirdly, it will force the sick poor, who wish to pay so far as they are able to pay, to abandon the struggle to help themselves, and compel them to what I think is degrading pauperism. That is not my own phrase, it is Dr. McGregor's. I would suggest first a provision for graduated payments in hospitals with adequate remuneration of the medical staff, and then the abolition of payment in hospitals when medical men give their services gratuitously. (Hear, hear.)

DR. RAMSAY (Launceston).—The only point upon which we may cross swords is the question of payments, and I am hoping that, as a result of this conference, some method will be brought forward of a constructive kind to prevent hospital abuse. The question of the payments cannot be considered apart from general policy. We have to determine what policy the hospitals of Australia and New Zealand are to be run on, and until the purposes and functions of public hospitals are strictly defined the application of the term "hospital abuse" lacks definition. Under existing conditions it is certainly unfair to the profession that certain patients who can pay are allowed to enter public hospitals and obtain treatment at little or no cost; but if the interests of the medical profession are properly safeguarded, is it right that the advantages of a properly-managed public hospital should be denied them? From the practical and scientific points of view, there are few of the profession who will deny that in cases of serious illness a well equipped and staffed hospital is the best environment for the treatment of disease, and that from the patient's point of view the best chance of recovery lies within its walls. The modern hospital is a costly institution, equipped with expensive fittings, pathological and clinical laboratories, operating theatre, &c., with its nursing staff under proper supervision and control, and with an ever-present resident medical staff. With such a well-engined charitable institution, the pauper patient has perhaps better opportunities of having his condition diagnosed and treated properly than the well-to-do patient who is treated in private practice. (Hear, hear). Is it not our duty as members of a liberal profession taking an altruistic view of our relations to the public, to see that every available means conducive to the comfort and health of any individual who becomes sick should be placed at his or her disposal independently of the financial position of that individual? Surely it is a matter that concerns solely the financial side of medical practice, and one can only consider it most unfortunate that questions concerning this aspect of our professional life should lead to much dissension amongst members of a respected profession. Why should we not fully throw open our public hospitals to any physically suitable applicant who seeks admission? If it be urged that the pocket of the medical attendant is frequently thus depleted by the loss of certain fees, I would answer that this condition should be rectified, and that the medical man should obtain adequate remuneration for services rendered. Thus the plea of unfair competition and hospital abuse would be nullified. My contention is that the public hospital system should be liberalized, so that patients of all classes may obtain admission, and these great institutions should become national ones to which any of the afflicted may turn. One of the features of the British hospitals commented upon by Professor Osler in his recent address at Cardiff was the absence of paying wards in connexion with the large general hospitals, and the opinion was expressed that the example given by America might well be followed by establishing such wards for paying

patients. The medical man would then receive his private fee, the hospital its adequate share, whilst any profit would go towards the maintenance of the poorer class of patients. Professor Osler's opinion has my hearty indorsement, and in a new country like the Commonwealth, where class distinctions are less marked than in the motherland, we may with advantage walk in the footsteps of our American brethren. The benevolence of the members of our profession in so freely giving their services gratuitously to the poor of the community requires no approbation from me, but the time has come when the profession should give some further consideration to its position in relation to hospital attendance. All honour to those who are able and willing to render such service. But looking at the matter without sentiment, is it right that ours should be practically the only profession where such service should be expected in an honorary capacity? And should not the State make some financial provisions for the remuneration of medical attendants on the poor? As to those in the intermediary position—either unable to pay the ordinary private fees, or to whom such payment would be a great hardship, they could be charged fees according to their means, and a due proportion of such fees be received by the medical attendant and by the hospital. Such patients should sign a statement as to their inability to pay full fees. The amount of payment could be determined by a finance committee, with a medical officer as adviser. The most equitable manner of adjustment would be for the values of the medical attendance and hospital maintenance respectively to be assessed, and each party to receive a proportionate amount of the total sum received. But such a method would be cumbersome, and it might be expedient to determine a percentage amount to cover all cases. Under the conditions of such a proposal as the above, the question as to the staffing of the public hospitals must necessarily be considered. The same liberalization should be allowed in the staffing as in the admission of patients. In all but the hospitals of the very large cities the medical men practising within reasonable distance of the institution should be permitted to attend patients if they so desire.

A DELEGATE.—I rise to a point of order. It strikes me this is a combination of public and private hospitals.

DR. RAMSAY.—Private wards might be established in connexion with public hospitals. That is the only question I desired to bring forward. It is a matter, however, in which I do not wish to set myself in antagonism with the majority of the profession; but I give my opinion, and will be guided by the opinion of others. (Applause).

DR. HOLLAND (Western Australia).—It appears in Dr. Worrall's motion that you are taking thought of the particular kinds of hospitals in the cities, and not taking thought at all for that great crowd of men who go to the back blocks and put up with horrible hardship especially in Western Australia. (Hear, hear.) There are a number of hospitals established over there, primarily by the Government. I think there are sixteen, and they are run by local committees. The Government subsidizes these hospitals on the basis of the contributions of patients. The usual thing is that they pay at the rate of 1s. per week, and the Government pays another 1s. towards the expenses. They get together a big association of people, some earning £10 or £20 a week, but they can all pay 1s. per week and take the benefits. They are entitled to treatment, not only in the out-door departments, but in their own homes. Not only that, but the rules of the hospital say that the subscribers are entitled to operations of every class. There is no reason why these hospitals should not continue to take the 1s.

a week where the patients are receiving a limited wage, say, up to £4 per week. It is impossible to continue without subscriptions from the public and the Government. The abuses could be dealt with in some way or other, but the main thing I want to point out is that if the resolution is carried to receive no payment whatever, it would mean closing a lot of these hospitals, which are absolutely necessary, particularly on the mining fields. (Applause).

DR. TAYLOR (Outtrim).—I am afraid we shall alienate the sympathy of a large section of the general public if it goes forth to the world that this Congress holds the opinions of the mover of the motion in regard to the causes of the trouble and the remedies. If I understood Dr. Worrall rightly, he was of opinion that the cause of hospital abuse was excessive charity and a plethora of hospitals. I do not think such is the real cause. The cause of hospital abuse is simply human nature. If the public see that they can get something very cheaply they will get it, or try to do so. We will have to go a long way before we get excess of charity. The proper remedy for this state of affairs is not the closing of a number of hospitals, but the education of the medical profession and of the general public. The proper step is being taken now in convening this meeting to discuss the question, and to ventilate what is necessary for the medical profession themselves and the general public. When the public realize that it is an inequitable thing for people who can pay to receive charity, the first step will be taken towards doing what is right.

DR. DOYLE (Newcastle).—I wish to support the addendum suggested by Dr. Barrett. Most of the people who go to hospitals go because they are unable to get the treatment they require elsewhere. In that connexion, I would like to refer to some of the statements made by the mover of the proposition with regard to the destruction of the self-reliance of the people by hospital treatment. In the first place, in discussing hospital matters in Australia, we are on totally different ground from what we would be if we were in older countries like England and Ireland. (Hear, hear). We are dealing here with people who are much better educated and better off. These people know where they get the best treatment. I am not connected at all with any lodge practice or club practice, nor am I attached at present to a hospital. The people continually come to me and say, "I want to have an operation done," or I suggest to them they should have an operation performed. I say, "Can you afford to go into a private hospital?" "No, I cannot." "What can you afford to do?" "I can afford to pay £1 or 30s. per week." Now, consider what £1 or 30s. per week is from the wages of the ordinary worker. (Hear, hear). We may take it the ordinary worker is within three or four weeks of starvation when he is incapacitated. Only recently I went into that question with a lot of patients, and found that the family of a wage earner when incapacitated and not on a lodge had to borrow or run into debt.

A DELEGATE.—That is a hospital case.

DR. DOYLE.—You talk about thrift on £3 a week and self-reliance. Treat him free certainly. As far as this resolution stands it is merely an academic expression of our opinion. It does not suggest to the general public or the Governments—who are, after all, the principal people to be considered, because they find most of the money for our institutions—any means by which this can be carried out. As Dr. Barrett showed you, immediately it was carried out in Victoria the hospitals would probably be

insolvent. The best thing would be to suggest to the Governments that no payment whatever should be taken from patients treated in hospitals, and in order to insure this all hospitals should be municipalized.

DR. BUTLER (Hobart).—Most of the members seem to have alluded to fees of from £1 to 30s. a week. In order to show the way in which this is a growing evil I would mention what occurred in Hobart and Launceston. The general hospital that I was first connected with was the Hobart Hospital. There was no such thing as payment of fees, except on a small scale, as a rule amounting to 10s. a week, which was looked upon by the board at that time as the amount it cost to keep those patients in hospital. Gradually the two boards got into a sort of competition as to who would collect most money, and by saving the Government most would get into most favour with the Government. That was the primary cause of increasing the charges. The maximum then became 3s. per day. That lasted some time, but now we have a very different state of things. In order to get some information, when I heard this matter was coming on, I moved for a return in Parliament of the number of patients in every hospital who paid 5s. per day or over. In the Hobart Hospital the number of patients admitted from 30th June, 1906, to 30th June, 1908, who paid 5s. per day or over, was 150. There was one who paid 10s. per day for ten days, one 10s. for twenty-one days. A great number of the paying patients of the Hobart Hospital were officers from His Majesty's Navy, who could not have been treated in another way, as it was the only hospital where we took them in, and their treatment does not interfere with the general practitioner at all. In a large number of cases these paying patients could afford more, if it was the taxing officer who had to decide and not the secretary of the hospital. (Hear, hear). In the Launceston Hospital there were 200 (I think it was about one-fifth of the total patients) who paid 5s. per day and over. Here the board seems to have a much better system of increasing their funds than we in Hobart, because I find one patient in 36 days paid £26 14s.; another in 47 days £64 7s., or rather over £1 per day; others in 21 days 16 guineas; in 37 days £26; in 14 days 19 guineas; in 15 days £22 10s., and several others. I hoped that I could have these returns printed in order to bring them over, but they will be printed shortly, and if those who take interest in these matters will give me their names I shall be glad to forward them copies of the returns when I have them. (Hear, hear). A man who goes into a hospital and pays a fee of that sort should be allowed to have his own medical man from outside, and that medical man should be allowed to charge for medical attendance. (Hear, hear). No doubt in this case the hospital is charging for medical attendance as well as for keeping. Even if the motion is modified in any way, it should be understood that no hospital charge should be made or payment received of any other amount than would pay for the patient's board and lodging. To carry the motion as it stands would not do in some of our country hospitals; and in our infectious diseases hospitals and other special hospitals patients can employ any medical men they like, and we cannot complain of any injustice as long as all the medical men in the town have free run of the hospitals to send patients in, and where it is thought fit the hospital should charge for their maintenance.

DR. CLARENCE READ (New South Wales) said.—Mr. President, Ladies and Gentlemen.—I would like to suggest a slight alteration in Dr. Worrall's motion, of which I am entirely in favour. The alteration would, I think, meet the case of New Zealand. If the word "General"

were put in before the word "Hospitals" in the second line, I think that would meet the New Zealand difficulty. The whole matter rests entirely in the hands of the medical profession. If we will only stand shoulder to shoulder, we can carry the resolution into practical effect. The trouble in the past has been that the medical profession has been divided. We, as medical men, have all been pulling different ways, and, in many instances, I am afraid, looking rather at our own personal interests. Dr. Barrett has asked if no money is received from the patients what would the hospitals do? I think Dr. Worrall answered that query when he said that the number of patients would materially be diminished. (Cheers). Several speakers have referred to "pauperizing the people." If a patient has a little, but cannot pay for his maintenance in the hospital, he can always give a donation, and I say that will remove from him the stigma of losing self-respect. In my opinion, if the hospitals allow people to go in and pay, a medical officer should be paid, and that raises a very large question, because there are a certain number of professional men who are fortunate enough to hold hospital appointments. I am thankful to say I am one of that class, but there is a large proportion of the profession who do not hold hospital appointments, and we must see to it that we do not feather the nests of the men holding public appointments to the disadvantage of the large proportion of those who do not. (Cheers). This must be borne in mind, and we must all be prepared to look at the matter in a fair and honest light. If we do, the matter will speedily be settled. (Cheers). One speaker referred to "alienating the sympathy of the public." If we have right on our side, why trouble about the sympathy of the public? (Cheers). Another speaker, Dr. Thomson, of Brisbane, referred to the fact that in many instances a surgeon will say, "My fee is so and so; if you cannot afford it, I will take you into a general hospital." Well, I think that is one thing which our leaders should seriously consider. I know that in London this practice is followed, and I think it might be followed out here. A professional man says, "My fee is 250 guineas for the operations." The patient says, "I cannot afford it." The professional man says, "Well, what can you afford? I will send you along to a surgeon who is a competent man and who is prepared to do the operation for you." The patient says, "I can afford 100 guineas." The professional man sends the patient along to a junior man, and the patient gives that junior 100 guineas. (Laughter). If you cannot get the 250 guineas, should you take the case into the public hospital and do it for nothing? (Cheers).

DR. LOCKHART GIBSON (Brisbane), said,—Mr. President, Ladies and Gentlemen.—I wish to say I am in very cordial accord with the motion as proposed by Dr. Worrall. Still, I think that we might modify and improve the three rules that we have before us in view of the suggestions made by Dr. Barrett. I think a verbal alteration of 3 and 4 would improve them in a way that would suit Dr. Barrett's views. It seems to me that there are three kinds of hospitals necessary—the public or general hospital, and it is to this that No. 1 rule should apply, and no payments whatever should be made by the patients to the general hospital. The next kind of hospital is a private hospital. We do not need to deal with that; but to meet our requirements there must be intermediate hospitals, such as Dr. Barrett suggests, and I think a small alteration in the provisions of No. 4 would meet that suggestion. With regard to payments by patients in country hospitals, that is very easily got over. One shilling a week from residents in the country districts to support the hospital in that district should be considered a subscription to the hospital, and not

payment for services to come. The entrance of a patient to a country hospital should be because he is a fit and proper patient to be treated in that hospital, and he subscribes his 1s. a week in order that the hospital may exist for the poorer patients in the district, and not necessarily for himself. It is quite as much a subscription to the funds of the hospital as the one guinea or the two guineas a year that the better paid patients subscribe. I do not think there would be any difficulty if an alteration were made to the rule in that direction. (Cheers).

THE HON. DR. NASH (Sydney).—Mr. President, Ladies and Gentlemen,—I think we have got somewhat into a tangle, and I would ask the President to put us right. I think we should affirm at this meeting that all public hospitals should be free. (Cheers). I ask you the question—Can these great hospitals be run as they are at present in this country on the money that the charitably disposed are prepared to give? The answer must be “No,” because the money is not to be found in the country for the purpose. The only bodies that will have weight in this matter are the Parliaments, who find the bulk of the money. The only thing I know of that will have weight with them from this Congress is the assertion of some definite policy—that the great hospitals run by the country should be run for absolutely poor people. (Cheers). The hospitals will never be run in the interests of the people who cannot afford to pay until the medical profession has unanimously declared that these hospitals must be run for people who do not pay one single shilling. (Cheers). Some of you might have heard a speaker who said that he knew of a case in the Sydney Hospital where somebody paid three guineas a week. Now, I have been fighting this battle on the board of the Sydney Hospital for some time, and this statement came as a revelation to me. It startled me to think of such a thing occurring in a hospital, and I promise you that the matter will be thrashed out when I get back. (Cheers). I am inclined to think that there is some mistake, although, unfortunately, I am assured that there is no doubt about it. I think this Congress should make a bold assertion to this effect, that the great public hospitals run on public money should only treat hospital patients free. That would be to the point, and I think it would prove effective. Why are public hospitals made so much of? The mover of this resolution quoted from leading papers in Sydney and Melbourne, which declared the startling fact that the death rate from sepsis in a maternity hospital was 1 in 1,000, while the death rate amongst the general public through the same cause was 1 in 100. Is not that a sufficient reason why any woman, self-respecting or otherwise, should be sent into a maternity hospital, where she has a much better chance of being saved. (Cheers). I am satisfied to leave my suggestion in the President's hands.

THE PRESIDENT then put Dr. Worrall's motion, and declared it carried with one dissentient.

DR. BUTLER (Hobart), moved—

“That, except in cases of accident or emergency, every patient should, before being treated, be required to sign a declaration of inability to pay for medical attendance; and that, so far as possible, all patients seeking treatment should bring a recommendation from their own medical attendant, whether lodge or other.”

He said,—Mr. President, Ladies and Gentlemen,—Now that the previous motion has been so thoroughly thrashed out, this one only comes in as a matter of course. I understand that in many cases these particulars are

already asked for, but they are not insisted on, and I think, as we have carried the resolution regarding having a free charitable institution, we should accept the second resolution as a means to obtaining that end. I therefore move it.

DR. CLARENCE READ (New South Wales) seconded the motion.

THE PRESIDENT put the motion, which was carried unanimously.

DR. JOHN THOMSON (Queensland) moved—

That this Congress affirms the foregoing resolutions as the foundations of any real reform in the present abuse of hospitals, and is of opinion that other hospital accommodation would extend naturally to the inclusion of all those able to pay for medical treatment.

He said—Mr. President, Ladies and Gentlemen—I have been invited to propose the third resolution, and I have to thank the convener for giving me this opportunity to express my views on Hospital matters.

For six years, from 1876 to 1882, I was Resident Surgeon of the Brisbane Hospital—we were not Medical Superintendents in those days. In 1876 I had charge of 130 beds with 1,106 in-patients, and in 1881, 204 beds with 1,189 patients. Since 1882 I have been continuously a visiting surgeon, and the in-patients last year, 1907, numbered 3,764, so I have experience enough to enable me to form some definite opinions, and in my presidential address to Congress, nine years ago, I expressed these pretty strongly.

My belief is, and I am confident most, if not all of you, agree with me that our hospital charities are very sadly abused.

Dr. J. K. Fowler's address in Medicine at the Sheffield meeting of the British Medical Association appears in the *Journal* of the 1st August. As an introduction to my remarks, I should like to repeat, with your permission, these sentences:—

“But how will the practitioner fare for a livelihood in the future if the ever-increasing tendency of the people to expect gratuitous medical attendance continues unchecked, and neither political party considers it to be its duty to encourage habits of thrift amongst the working population?”

“For thirty years, to my knowledge, the abuse of hospitals has been a burning question, and many earnest men have given time and thought to its consideration; yet nothing of any moment has been done.

“This abuse of the generosity of the profession will, in my belief, continue and increase so long as it is to the interest of the managers of hospitals to show that they are doing what it is the fashion to call ‘a great work.’

“Having enjoyed a somewhat extended and varied experience of hospital committees, I have learnt to admire the devotion of their members to a self-imposed duty of ever-increasing difficulty, but this does not dispose me to blindly trust to their wisdom. The moment that the relief of mere numbers ceases to be a merit, and it becomes to their interest to show that they have done everything possible to insure that those who receive gratuitous medical treatment are really deserving of it, a great falling-off in the attendances will immediately occur.”

I recognise four different kinds of hospitals:—

1. Infectious Diseases' Hospitals.
2. Public General Hospitals.
3. Intermediate or Provident Hospitals.
4. Private Hospitals.

I. INFECTIOUS DISEASE HOSPITALS.

Hospitals Provided and Maintained at the Government Expense.

The hospitals so maintained would naturally include Hospitals for Incurables and Hospitals attached to Benevolent Asylums and Asylums for the Insane, but the institutions to which I wish to refer are those created under Health Acts, and known as

Infectious Disease Hospitals.

While I freely admit that the official intention is that the patient admitted to such an institution shall receive the best possible treatment, the fact still remains that in many instances, he has been removed not for his own sake, not for the attentions he is to receive—he could get these at home, get them better perhaps—but for the public weal. He must suffer—surely a compulsory isolation is a form of suffering—that infection may be stayed, and the public health conserved. Then they who benefit should pay; if the public are protected, then the costs should be a charge upon the public purse. The individual might be expected to purchase any luxuries he desired or was ordered; these he would have to provide at his own expense at home, and it would be no hardship to do the same in hospital.

It might be argued that the same holds good generally, that his illness, even if treated at home, might be costly, and it would be no particular privation for him to be at the same expense when looked after by the health authorities. No, the one is voluntary, he has his own doctor, his own nurses, his own home comforts and surroundings; the other is compulsory, and with it all the unpleasantness, real or imaginary, which compulsion, in any form, invariably creates. If he is not a martyr, he thinks himself one, and to make him pay suggests a double penalty, fine and imprisonment.

II. PUBLIC GENERAL HOSPITALS.

These should be benevolent institutions in the strictest meaning of the term—none but the sick and suffering poor should be admitted. Technically, they should provide for the immediate, and, if necessary, prolonged care and comfort of all those who, requiring medical or surgical assistance, are unable to pay for it, and render "first aid," or undertake the emergent treatment only of urgent cases among all classes.

At present they are maintained by private and public subscriptions, donations, bequests, endowments, and Government assistance.

There should be no scale of charges—no patient should be billed, and no payment for services rendered, should be received from any one.

Allowing for what might be called permissible irregularities, and in spite of the vigilance of economic secretaries, and the valuable aid of Charity Organization Societies to protect the hospital against imposition, a vast amount of abuse does take place, and the "out patient" department is responsible for a very considerable—if not the major—share of this.

Attempts have been made to regulate the admission of patients "in" and "out" on a wage limit basis. I fancy these attempts have failed. I do not see how it could be otherwise. One man is passing rich on £100 a year, another is a bankrupt and a pauper on the same amount. A single man with £200 a year should be quite ineligible; a married and family man, with £300 a year, might be very justly entitled to all the benefits of the institution.

I hold very strongly to the idea that no patient should pay. All should be admitted on the same terms. All should realize that they are the recipients of the bounty of the benevolent, or, to put it as it used to appear on the old admission ticket, that the individual is "a fit subject of charity" (Rules, Brisbane Hospital, 23rd June, 1873). This term was offensive, and abolished after some ten or twelve years use, but "Uttered not, yet comprehended" it had its value, and by putting all on an equal footing, many, perhaps most, who salved their pride by flattering themselves they were paying patients, will cease to trouble.

In New Zealand—among other sources of revenue—grants are received from contributory local authorities, proportional to the rateable value of property in the district. It might be an addition to the Income Tax; it might be a Hospital Tax, pure and simple. This would at first be hideously unpalatable, but like many nauseous medicines, its effects would be "searching." It would not fall on those now cheerfully and liberally giving—it would probably relieve them—but it would catch most of those who can subscribe, but don't and won't.

Nationalization, if it did not abolish existing unnecessary hospitals, would certainly prevent the establishment of new ones—some only to perpetuate a name, some to humour a fad; it would control overlapping; construct the proper machinery for the careful expenditure of the vote; check abuses, and punish offenders.

It has been said that nationalization must necessarily carry with it free medical attendance and medicines for all and sundry: that as the State is the national schoolmaster providing free education for all its children, so it would become the national doctor, controlling and treating

"The heart ache and the thousand natural shocks
That flesh is heir to,"

and establishing free medical halls for all who choose to flock to them. I have no fear of this. It might be the dream of a visionary, it will never be the Act of a Parliament.

III. INTERMEDIATE OR PROVIDENT HOSPITALS.

There must be many patients, not poor enough to require the charity of a public hospital, and not rich enough to pay the charges of a private one, who would welcome the advent of an intermediate institution, where, even admitting a sliding scale of fees to suit individual requirements, the bill included all professional, nursing, and hospital expenses.

Attempts have been made from time to time to establish such, but with what success I hardly know.

In Brisbane, the Friendly Societies have a hospital of their own; the *Australasian Medical Gazette* of August reports an institution of the kind in Carlton, and I understand that in England, notably at Northampton, some are working satisfactorily.

If the public hospitals are nationalized, and all payments of patients stopped, there will be a demand for these intermediate institutions, and they will spring into being.

How they ought to be constituted I can hardly advise. While they should be under Government supervision, I doubt if they should be Government institutions. They might be a further extension of the work of the Friendly Societies—they might be branches of Life or Accident Assurance Companies—they might be co-operative efforts—or private ventures. But whatever way, the payment of the medical attendant is part of the scheme, and in arranging this, professional etiquette must be satisfied, and it just possible the British Medical Association may have to regulate what will be practically a new development of hospital growth.

IV. PRIVATE HOSPITALS.

All hospitals, certainly all private hospitals, as in South Australia and New Zealand, and all intermediates, should

1. Be registered or licensed after the authorities are satisfied that:—

- (a) The buildings are suitable.
- (b) Fire extinguishing appliances are installed.
- (c) Fire escapes are provided.
- (d) The sanitary arrangements are satisfactory.

I know certain houses, used as hospitals, which would not fulfil these requirements.

2. Be open at all times to inspection by a qualified officer of the Health Department.

3. Keep a register, among other minor details, of:—

- (a) All induced anæsthesia.
- (b) The operation for which an anæsthetic was given.

I think this is essential. Some dozens of women have consulted me, during the last few years, all with the same story—they report an operation—they show an abdominal scar—some want to know if they have been unsexed—some if they have been mutilated—many declare that whatever was done, did no good—and all profess absolute ignorance of the nature or extent of the operation. The scar may some times suggest, it seldom speaks!

When a woman leaves a hospital, she should be given a certificate reporting the operation she underwent, and an official record should be kept in the institution.

4. Bring all their nurses under the Factories and Wages Act.

Surely there cannot be much objection to this. Many of the young probationers work for very long hours, and for very trifling wages. In the larger institutions the hardship is not so keen, there is compensation in the skilled and technical training they receive, qualifying them for certification and registration; in the smaller hospitals there is no compensation, for there can be no qualification.

DR. W. ANSTEY GILES (South Australia) in seconding the motion said:—"Mr. President, Ladies and Gentlemen,—As this discussion has lasted so long, I do not think many words from me will be necessary. The entire movement has my support and sympathy, and there is no question but that the time is ripe for this important matter to be thoroughly

ventilated by Congress; and I trust, if we do not entirely effect a cure of these abuses under consideration, yet that there will be a considerable amelioration, and that we shall be able to insert the thin end of the wedge and gain some advantage for the profession. It is a burning question in my own town. Some years ago it happened that the great body of the profession was unable to avail itself of the advantages of a general hospital, and then it became necessary for us to make some arrangements whereby we could attend to a certain class of our patients. The private hospitals generously gave us considerable aid. A considerable modification was made in the ordinary scale of fees, and patients who deserved special treatment received it. I was very much surprised at the number of individuals whom I would otherwise have been called upon to treat at the Adelaide Hospital, who were able to scrape up sufficient to pay the private hospitals' modified fees, and the small demands for remuneration which I made upon them. Still, we worked along very happily under those conditions. The patients were not pauperizing themselves, and the medical men, who gave their services, received certain remuneration, of course, not compensating them by any means for the work done, but still satisfactory enough. The question of separate hospitals being established of a secondary character does not appeal to me in any way. I think the general hospital should certainly be for the undoubted poor. I think the private hospitals, which are multiplying in our midst, will be anxious to keep their wards and special rooms filled, and will be only too pleased to make certain arrangements with the medical profession, whereby patients, deserving special consideration, can obtain it. (Cheers). The whole matter really, as far as this resolution is concerned, requires to be carefully deliberated upon by a special committee, and I trust that committee will be able to formulate some scheme which will appeal to the general profession, and be of distinct advantage. (Cheers).

DR. LOCKHART GIBSON (Brisbane) said:—"Mr. President, Ladies and Gentlemen, might I suggest a verbal alteration? I venture to hope that perhaps the mover and seconder may accept it. It is this:—Add after the words, "Would extend naturally to the inclusion of all those able to pay," the words "fully or partly for medical or surgical treatment" instead of the words "for medical treatment."

The mover and seconder, offering no objection to the alteration, the motion was amended accordingly.

DR. ARGYLE (Victoria) said:—"Mr. President, Ladies and Gentlemen,—In regard to the question of intermediary hospitals, which appears to be the great crux in the solution of the hospital question throughout the whole of Australasia in the large centres—the question of dealing with people who are able to pay something and yet not able to pay the ordinary fees of a private hospital—I would like very briefly to suggest a possible way of arriving at a solution of this very real difficulty. I would suggest that a way of getting at the insurance proposal referred to by the mover of this resolution might be obtained by collecting insurances by means of the Government through the Income Tax of people whose incomes range between £100 and say £300 or £400 per annum. The intermediary hospitals might then be maintained as institutions run by the Government for those insured who were ill, either medically or surgically. The private medical officers of those sick could attend in these institutions for fees arranged between them. It seems to me that if some such system could be adopted, many difficulties now existing could be removed.

THE PRESIDENT announced that Dr. Moore wished to submit some resolutions agreed to at a conference of delegates representing the Victoria Branch of British Medical Association, the Medical Defence Association of Victoria, and the Melbourne Medical Association.

DR. W. MOORE (Victoria).—Mr. President, Ladies and Gentlemen, — With regard to the resolution now before us, I may say that the Committee gave very great consideration to it. I am glad Dr. Lockhart Gibson has been able to improve it for us, because certainly we read it over and over again, and we were individually quite satisfied with it. One thing, however, we are sure about, and that was that we were not in a position to make any suggestion as to the starting, or even the approval of the starting, of intermediate hospitals, but we thought that the resolution, as we worded it, left the matter open for any developments that might occur. Personally, I still think that is by far the wisest plan in regard to the development of intermediate hospitals. I do not think that it is for us to say that they should be started. A little over a week ago, delegates from the Victoria Branch of the British Medical Association, Medical Defence Association, and Melbourne Medical Association, met and discussed the question of the conditions under which the medical profession might approve of intermediate hospitals, if anybody started them. I will read the resolutions arrived at by that conference, and my reason for so doing now is because there are so many visitors from the adjoining States and New Zealand, that I think it might be of use to them to know what that conference, after very careful consideration, decided to do. I have the acquiescence of Dr. Syme, who was in the chair when these resolutions were affirmed, to read them to this Congress. They are as follows:—

1. That intermediate hospitals shall be for patients who are ineligible for public hospitals, but who are unable to pay the customary fees, as well as the charges usually made in private hospitals.
2. That there shall be no medical staff, but that any medical man may send in patients and attend them.
3. That the medical profession shall be adequately represented on the management.
4. That no patient shall be admitted without first producing a certificate of suitability for admission from a medical practitioner, who should, whenever possible, be the patient's usual medical attendant. If the signatory be not the patient's usual medical attendant, an explanation of the fact should be appended to the certificate.
5. There shall be no out-patients.
6. That the same charge for maintenance shall be made to all patients in any one hospital.
7. That the professional fees shall be arranged between the medical attendant and the patient.
8. That touting for patients by advertisement, or otherwise, shall not be permitted.

THE PRESIDENT then put the motion amended as suggested by Dr. Lockhart Gibson, and same was carried unanimously.

MR. G. A. SYME (Victoria), in the absence of the mover of the paper, Dr. W. Chisholm (Sydney), moved :—

That this Congress is of opinion that it would be a great gain to the community if an executive body were established to—

- (a) Administer the Government grant.
- (b) Classify hospitals.
- (c) Prevent overlapping of hospital work.
- (d) Insist on inquiry for the prevention of imposition.
- (e) Insist on the adoption of uniform methods of keeping accounts.
- (f) Close unnecessary hospitals.
- (g) Prevent the establishment of unnecessary hospitals.
- (h) Foster economy in hospital administration.
- (i) Promote the transfer of patients from one class of hospital to another.

He said : I do not think it is necessary to say very much in support of this resolution. It covers some of the objections which have been raised in discussing the first resolution, and it also deals with matters which have been discussed in the public press and meetings of various medical societies from time to time. The motion speaks for itself, and therefore I do not propose to detain you, and I now simply desire to move it.

DR. CUSCADEN (Victoria), seconded the motion.

DR. LOCKHART GIBSON (Queensland).—May I suggest a verbal alteration? I wish to meet Dr. Barrett's requirements for an independent inquiry, and it seems to me that a verbal alteration in the opening words of the motion would meet the case, so that the motion would commence as follows :—"That this Congress is of opinion that it would be a great gain to the community if an executive body, independent of any one hospital, were established."

DR. SYME.—I would like to add myself the words, "with statutory power." That would be necessary to enable the administration of the Government grant.

THE PRESIDENT.—Is that allowed by Dr. Cuscaden? (Voices : He has gone). Does the meeting allow it? (Cries of "Yes"). Then the alteration is made.

DR. SINCLAIR GILLIES (New South Wales).—Mr. President, Ladies and Gentlemen,—I would like to ask what is to be the constitution of the executive body, and who is to elect that body? Is it to be a stated body, or a body elected by the medical profession?

THE PRESIDENT.—There will be a contingent motion after this, providing for the creation of a committee to thrash out details, and to report through the next executive to the next Congress.

A DELEGATE.—Does that mean that nothing will be done till the next Congress?

THE PRESIDENT.—There will be also another resolution to send these different motions to the different medical societies and organizations in Australasia, and to ask them to take whatever action may be necessary in the interim.

DR. ANSTEV GILES (South Australia).—May I propose a further amendment? It is this : "That this Congress proceeds to elect a committee to deal with and forward the objects of this resolution."

THE PRESIDENT.—That will be done. I will now put the motion.

The motion was carried unanimously, the introduction reading as follows :—

“ That this Congress is of opinion that it would be a great gain to the community if an executive body, independent of any one hospital, were established with statutory power to ”

THE PRESIDENT asked Dr. Moore (Victoria) to move a contingent resolution.

DR. W. MOORE (Victoria). said he would like to give notice of motion for Saturday next, when a committee could be appointed. He said :—I wish on Saturday at the general meeting to submit two resolutions : (1) That a copy of the resolutions passed this morning be sent to all the medical societies in each State and in New Zealand, and that they be asked to take any action that they may think desirable in regard to them. Also, (2) that a committee be appointed to co-operate with the next executive committee in dealing with questions concerning the management of hospitals, and to report through that Executive to the next meeting of Congress. In regard to the names, I will be prepared to give them on Saturday.

THE PRESIDENT.—That is a notice of motion then for the final general business meeting on Saturday next. That closes the first matter before us.

The text of the resolutions as adopted by the Congress was as follows :—

1. That in the opinion of this Congress no payment whatever should be taken from patients treated at hospitals maintained by the donations of the charitable and by Government grants.
2. That, except in cases of accident or emergency, every patient should, before being treated, be required to sign a declaration of inability to pay for medical attendance; and that as far as possible all patients seeking treatment should bring a recommendation from their own medical attendant, whether lodge or other.
3. That this Congress affirms the foregoing resolutions as the foundation of any real reform in the present abuse of hospitals, and is of opinion that other hospital accommodation would extend naturally to the inclusion of all those able to pay fully or partly for medical or surgical treatment.
4. That this Congress is of opinion that it would be a great gain to the community, if an executive body, independent of any one hospital, were established with statutory power to—
 - (a) Administer the Government grant.
 - (b) Classify hospitals.
 - (c) Prevent overlapping of hospital work.
 - (d) Insist on enquiry for the prevention of imposition.
 - (e) Insist on the adoption of uniform methods of keeping accounts.
 - (f) Close unnecessary hospitals.
 - (g) Prevent the establishment of unnecessary hospitals.
 - (h) Foster economy in hospital administration.
 - (i) Promote the transfer of patients from one class of hospital to another.

CONTRACT-PRACTICE ABUSES.

DR. A. PALMER (Sydney).—Mr. President, Ladies and Gentlemen,—The resolutions I am about to put forward are those framed by Dr. Adam Dick, of Sydney. I now beg to move —

“That no medical man should pass for admission into any friendly society, lodge, club or association, for professional attendance at contract rates of remuneration, any person whose income from all sources, together with that of his wife (if any) exceeds £4 per week, that is, £208 per annum. Further, no medical man should attend professionally at contract rates any member of any of the above-mentioned organizations, whose income from all sources, together with that of his wife (if any), exceeds the sum of £6 per week, that is, £312 per annum.”

Dr. Dick was a member of the Council of the British Medical Association of New South Wales, and had given up a large amount of his time to the consideration of contract practice. He was not able to be present at this Congress, and therefore I am representing him here. I have not come prepared to make any long speech on the matter, and I would leave it to members present to fully discuss it. There are two or three points which Dr. Dick has asked me to emphasize. For instance, there can be no doubt about the necessity of this reform. If reform is necessary for hospitals, it is just as necessary for lodges and friendly societies. The profession does so many things for nothing, and so much for the poor, both in their homes and in hospitals, that there is really no reason why they should do the same for people who are able to pay proper and fair fees. These resolutions, I may say, have already been affirmed in a conference between the Friendly Societies Association and a sub-committee of the Council of the British Medical Association of New South Wales. There was only one dissentient in that conference. It is quite true that since that conference many of the lodges have refused to adhere to the agreement arrived at by their own delegates. It is a fact, however, that in many of the country districts of New South Wales the wage limit has been recognised. It is also a fact that the largest lodge in New South Wales has agreed recently to these resolutions. (Cheers). No one can accuse the medical profession of uncharitableness; but when persons, who have incomes beyond what is really proper for contract practice, get into lodges and secure medical attendance at contract rates, they are virtually robbing the medical profession. It is not intended by Dr. Dick to make the matter retrospective. He suggests the 1st January, 1909, as the date when the new rule should come into operation. Of course, this Congress merely expresses an opinion. It cannot insist on the observance of the rule. It would help matters a great deal if Congress would affirm this principle, and then a copy of these resolutions could be sent on to the various States. Of course, each State must decide for itself the best way to bring about a satisfactory result. (Cheers).

DR. SANDES (Sydney) seconded the motion.

DR. MARTIN DOYLE (Newcastle) suggested that the words “private list” be added after the word “association” in the first part of the motion. He said:—There is a peculiar condition of affairs existing in some parts of New South Wales, where persons, no matter what their incomes may be, pay from 6d. to 9d. a week, these payments entitling them and the members of their family to medical attendance. It

came under my notice recently that two medical men in Newcastle were attending a man whose income was £1,200 a year. That same man's will was recently proved at £16,000, and yet that man received medical attention for ninepence a week. If the words "private list" were added it would meet such a case.

THE PRESIDENT said that Dr. Doyle's amendment was that the words "Private list" be added to the resolution, so that the words would run—"That no medical man should pass for admission into any friendly societies, lodge, club, association, or private list, for professional attendance, &c., &c. Did the mover agree with it?"

DR. PALMER.—I have no objection.

DR. CLARENCE READ (New South Wales).—Some definite action should be taken in regard to this matter without any loss of time. It is no use going on until we are engulfed. In Victoria apparently things are in a sad way with regard to Lodge practice. We in Sydney have to a large extent overcome this evil. If you will only take a leaf from our book, you will take steps to remedy it at once. It is no good waiting. The longer you wait the worse it will get. (Cheers). About two years ago a committee of the New South Wales Medical Association was appointed to confer with the Friendly Societies. We had two meetings, and came to a definite conclusion. A compromise was arrived at between the two sides. We asked the Friendly Societies' Delegates to appear before their respective associations and to give us their final reply. They found, however, that they could not give us a reply until after twelve months. They undertook on their part to do their utmost to get the Friendly Societies to adopt the Model Lodge Agreement, and we on our part agreed to endeavour to get the Medical Profession of New South Wales to adopt it. What happened? At the end of twelve months, when we asked them what they had done, they had done nothing. They had simply shelved the question. That is the way Friendly Societies generally treat the medical profession. During the last few weeks, our Council has received a notification stating that the Friendly Societies refuse to have anything to do with the Model Lodge Agreement, with the exception of a few which have been brought to their knees by what you may call actual strikes. Then a crisis followed, and the medical men gave up the lodges, with the consequence that some blacklegs were introduced. You will be pleased to hear, however, that our men practically won. (Cheers). In Newtown, the Loyal St. John's Lodge, one of the most powerful lodges in the State, has agreed to the Model Lodge Agreement. Before they did so, its members paid nothing for accouchement cases, but under the agreement payment is made. Now, this was brought about by co-operation, by the medical men sticking together, and the only way you will be able to do it in Victoria is to follow the same tactics. We must all be prepared to stand shoulder to shoulder, and we shall win the day. I am in hearty accord with the motion.

THE PRESIDENT put the motion with the amendment suggested by Dr. Doyle, and it was carried with acclamation.

DR. A. PALMER (New South Wales) then moved for Dr. Dick, the second and third resolutions, as follow:—

2. That the General Secretary communicate with all the Medical Associations throughout Australasia, in order to bring into operation the foregoing resolution.

3. That, owing to the importance of this subject to the medical profession, the Presidents of future Congresses shall present brief reports to Congress upon the state and conditions of contract-practice, and that at future Congresses there shall be a section for the consideration of such Presidential report (and similar subjects), and that the chairman of such section be the General Secretary of Congress.

THE PRESIDENT expressed the desire that the third resolution should be amended. The present Congress could not well do more than make recommendations to the Executive Committee of the next Congress. It would not be wise to put the duty of reporting concerning any specified subject upon a future President of Congress. He might not have the knowledge necessary for him to deal with it. It would be far better to ask that it be done by or for the Executive Committee. He therefore suggested that Resolution 3 should read as follows:—

3. That, owing to the great importance of the subject to the medical profession, the Executive Committee of the next Congress be requested to present a brief report to Congress upon the state and conditions of contract practice, and to take such further action as it may deem advisable.

THE PRESIDENT thought that the new Executive would appoint a Special Committee to deal with the matter, and the various resolutions would be in the hands of the Medical Associations throughout Australasia, which would consider the matter, and take such action as might be deemed advisable.

DR. PALMER accepted the amendment of Resolution 3, suggested by the President.

THE PRESIDENT then put Resolution 2 and Resolution 3 as amended, and declared them carried unanimously.

CORRESPONDENCE.

THE PRESIDENT announced that he had received the following communications:—

- (1) From Miss FitzGerald, asking him to accept for himself as President, and for all the members of Congress, the heartfelt thanks of her sister and herself for the kind expressions concerning the death of her father, Sir T. N. FitzGerald.
- (2) From the President of the Council of Churches of Victoria, conveying the following resolution:—

“The Congress of the Council of Churches sends cordial greetings to the Medical Congress now sitting in our city, and trusts that its deliberations will not only much promote the progress of medical science, but also advance the better health conditions of our communities, and above all aid in the raising of the moral standard of our people's life.”

The President said that he had replied, reciprocating the good wishes expressed in the letter.

- (3) From the Mother-Rectress of St. Vincent's Hospital, inviting members of Congress to inspect the Hospital.

- (4) From Mr. John McLellan, inviting members of the Congress to be present at the Exhibition of Physical Culture by the members of the Girls' Guild, in the Melbourne Town Hall. The President spoke in terms of high appreciation of the work that was being done by the Guild under the presidency of Mr. McLellan.
- (5) From the Women's Christian Temperance Union of Australasia concerning the "non-alcoholic medication" department of their work, and the instruction of the young in temperance principles. From the corresponding Union of Victoria, forwarding a manifesto by the British Medical Temperance Association, and asking for an expression of opinion by Congress.
- (6) From the Executive of the Odontological Society of Victoria, drawing attention to its efforts to have the teeth and associated structures of State school children periodically examined by competent medical practitioners; and stating that in a number of schools they had found only 4 per cent. of children at an average age of thirteen, free from caries.

The President said that he was sending a sympathetic reply on behalf of the Congress.

ADJOURNMENT.

THE PRESIDENT then declared the Congress adjourned.

GENERAL MEETING OF CONGRESS.

WEDNESDAY, OCTOBER 21ST, 2 P.M.

(WILSON HALL). •

The Congress re-assembled in the Wilson Hall on Wednesday afternoon, when the Address on Public Health and State Medicine was delivered by the President of the Section, J. M. Mason, M.D., Brux., L.R.C.P., et S.Ed., D.P.H., Cantab., Director of the Department of Public Health and Chief Health Officer of New Zealand, the subject being "Some Side-lights on the Work of a Health Officer."

THE PRESIDENT of the Congress, who presided, expressed the thanks of the Congress to Dr. Mason for his instructive and illuminative address, which will be found at the head of the proceedings of the Section of Public Health and State Medicine.

THE CONGRESS DINNER.

WEDNESDAY, OCTOBER 21ST, 7.45 P.M.

(MELBOURNE TOWN HALL.)

The Victorian members of the Congress entertained the visiting members and other guests at dinner in the Melbourne Town Hall on Wednesday evening, October 21st. The President (Professor Allen) occupied the chair, having on his right hand His Excellency the Governor of Victoria (Sir Thomas Gibson-Carmichael, Bart.), and on his left the Lieutenant-Governor (Sir John Madden, G.C.M.G., C.J.) At the high table were the Lord Mayor of Melbourne (Sir Henry Weedon), the Minister of Education (Mr. Sachse), Captain Creswell (Naval Director), Mr. Justice a'Beckett, Mr. Robert Harper, M.P., Archdeacon Hindley, Dean Phelan, Sir George Jenkins, Mr. C. E. Bright (Deputy Postmaster-General), the past Presidents of Congress, the Presidents of Sections, the Deans of Faculties of Medicine, Surgeon-General Williams, Surgeon-General Skerman, Fleet Surgeon J. Falconer Hall, Dr. Cahill (President of the New Zealand Branch of the British Medical Association), the representatives of State Governments, Dr. J. F. C. Mackenzie (representing the R.A.M.C. in India), the Presidents of other Learned Societies, the Town Clerk (Mr. John Clayton), the Chief Commissioner of Police (Mr. O'Callaghan), and others. The vice-chairs were occupied by Dr. J. Jamieson, Dr. J. W. Barrett, Mr. F. D. Bird, Dr. A. J. Wood, Dr. R. R. Stawell, Dr. Moss, Dr. Beattie Smith, Dr. G. R. Adam, and Dr. W. P. Norris. Apologies for non-attendance were received from the Prime Minister (Mr. Deakin), and the Premier of Victoria (Sir Thomas Bent), and other members of the Victorian Ministry, who were detained by the sittings of the Parliaments, from the Chief Justice of the High Court (Sir Samuel Griffith, G.C.M.G.), the Bishop of Tasmania, Colonel Wallack, Colonel Stanley, and many other gentlemen who were unable to be present. About 400 sat down to dinner.

THE PRESIDENT proposed the toast of His Majesty the King, which was received with enthusiasm, the members joining in singing the National Anthem.

DR. F. ANTILL POCKLEY (Sydney) proposed the health of His Excellency the Governor of Victoria in a loyal and humorous speech. In one particular respect, he said, His Excellency appealed to medical men, and that was that he was a distinguished worker in a branch of science of which they all had a smattering—natural history. (Applause.) As a compatriot of Robert the Bruce, it was only fitting that His Excellency should have devoted considerable time to the study of spiders. (Laughter). He ventured to think that one reason His Excellency had in coming to Australia was that he might have a better opportunity of becoming acquainted with such animals as centipedes, though, for his own sake, he hoped they would not become too intimately acquainted. (Laughter and applause.)

HIS EXCELLENCY THE GOVERNOR (SIR THOMAS GIBSON-CARMICHAEL), in responding, said he felt rather like the woman of Samaria. Dr. Pockley had told them about everything he had done, and apparently he had done nothing that was wrong. Dr. Pockley had begun by referring

to some archaic joke that the medical profession ought never to drink the "health" of any one. He did not agree with that at all. He thought if his health were good while he was in Victoria it would be entirely due to the medical profession. (Laughter.) On arriving in Victoria the first thing he had done was to go into a hospital to be treated. (Laughter.) He was not going to confess how many medical men had treated him since he had come here, but he had derived benefit from their treatment—at least he hoped so. (Laughter.) Therefore, he thought they were perfectly unselfish in drinking his health. Dr. Pockley had referred to his (His Excellency's) interest in spiders and centipedes. Well, he did take an interest in them, and did not see why he should not. (Hear, hear.) He hoped a great many other people in Australia would take an interest in them. He would admit that his interest in naturalist studies was one of the things that appealed to him when he was asked to emigrate. (Laughter.) He had received a telegram from the Secretary of State for the Colonies asking whether he would be prepared to emigrate. He had replied that it all depended upon where it was to. One of the things in his mind at the time was that he would only emigrate to a country where centipedes and the like were interesting. (Laughter.) He did not know whether the fact was of interest to the doctors, but he might tell them that he had been bitten by a bull ant. (Laughter.) He had read in a newspaper that he was late in arriving at a gathering because he had been stung by a bull ant, so he thought he would try what such a bite was like. (Laughter.) As a matter of fact, he had had more pain inflicted upon him by insects in the old country. Dr. Pockley had said that the labours of a Governor were very heavy. He was not going to deny it (laughter) but he was going to discharge those duties. So far, he had found everything very pleasant. The worst feature about them was that a State Governor was expected to make speeches. He knew that in Australia there was a high standard of eloquence, and he felt doubtful whether he could live up to it. He had felt grateful to read in the newspaper, which he was told was more widely read than any other newspaper in Australia, that no Governor had ever come to this country yet who was such a poor orator as himself. (Laughter.) He felt grateful to the man who had written that paragraph, and to the editor who had inserted it, because he knew that nothing would be expected from him, and, therefore, if he scored in any way, it would be unexpected, and, therefore, the more delightful. (Renewed laughter.) He did not intend yet to try to score. He hoped they would go on considering he was a Governor, a State Governor—they knew the difference. As he was not an Australian, he need not express his views on State Governors—(laughter)—but as long as he was State Governor he would try and perform his duties. (Hear, hear.) He was a State Governor who took an interest in spiders and centipedes. He would do all that was necessary in laying the foundation stones of churches or presiding at microscopic entertainments, which required more or less intelligence to perform properly. (Laughter.) He would go to medical congresses and try to discharge his duty, but he did hope that every one in Victoria would go on considering he was not a State Governor who knew how to make speeches. (Laughter and cheers.)

THE LIEUTENANT-GOVERNOR (SIR JOHN MADDEN) said that, by a fortunate occurrence for him the Prime Minister of Australia had a congress of his own to attend that night—one far less agreeable than that at the Town Hall—but it had given him (Sir John Madden) the opportunity of assuming the high honour of proposing the toast of the Medical

Congress of Australasia. All of us had a deep interest in the medical profession. It would be well remembered that members of that profession had for the most part assisted to bring all of us into this world; they had also to a great extent enabled us to continue in it with more or less success, and they had helped us out of it—almost precipitately sometimes he was bound to say—(laughter)—but always more comfortably than the exit could otherwise be expected to be. In the next place, he assured them of the distinct gratification we all felt in finding in our city a body of gentlemen whose names and good works and reputation had long preceded them. We were also glad of the event which brought them here, because, undoubtedly, their work could not but be a matter of great importance to Australia, and possibly to the world at large. It was said that when two or three were gathered together in God's name He would grant their requests. Medical men had many things to ask from God in the midst of the thanksgiving they owed Him, and it might be that those who formed the Congress would be able to formulate the prayer which was necessary, and obtain its answer. What Congress was there that stood so much in the name of God as this gathering together to take means by common devices and consultations to preserve human life and alleviate or avert human suffering. Judging by the achievements of the past twenty or thirty years there was a great vista of possibilities open to the trained medical men of to-day. [The Lieutenant-Governor here gave a most humorous sketch of the developments that might flow from recent experiments in the transplantation of organs.] Then, continuing, he noted that the President in his inaugural address had said that life itself was a series of fermentations. The ferment which constituted the life of all organized things might reverse the action of the death-bringing ferment, and the death-bringing ferment might reverse that of the life-giving ferment. The outcome of the study of this struggle might possibly be to avert death altogether, but, even if this should not be attained, life might be greatly prolonged by successful treatment. An American philosopher had said—"Life is a mystery as deep as death can ever be." The Lieutenant-Governor then referred to the experiments of Loeb in causing fertilization of the eggs of sea-urchins by chemical treatment and arresting development by varying the chemical medium surrounding the ovum; so that it seemed as if life could be managed at the will of the experimenter. The lay public expected great things from these discoveries, but members of the medical profession would be better able to measure them at their true value. At this present day, with the four "A's"—anatomy, anti-sepsis, asepsis, and audacity—(laughter)—they could equip an up-to-date surgeon. In his young days, they had skilful surgeons who knew all about anatomy and audacity, but nothing about the other two "A's." How was it they did not fill the cemeteries? (Loud laughter.) The only reason he could give was the Irishman's—"They had the devil's luck and their own." The old-time physicians groped their way through darkness to success, and the practitioners of the present day might be asked—"Are you making the best of the blaze of knowledge which surrounds you on every side?" It was an axiom of tuberculosis that human sputum was a disseminator of this great disease. If they controlled that disseminator they could look for the disappearance of that great scourge. (Hear, hear.) We were making regulations to control it, and we were building sanatoriums, yet in Melbourne, when a north wind blew, there came a Sahara of dust laden with tuberculous germs, which was thrust down our throats. We wanted to overcome that fault. It was said by the highest authority

that "Dust ye are, and to dust ye shall return." We surely took active steps to acclimatize ourselves, and to accustom ourselves to our future state. (Hear, hear, and laughter.) It was said it would cost money to remedy this state of things, but the lives that would be spared and the health preserved for the rest of the community would far outvie the money spent on such a purpose. (Applause.)

THE PRESIDENT (PROFESSOR ALLEN) replying to the toast of "The Congress," spoke as follows:—Your Excellency and Gentlemen,—On behalf of the Congress, for the members who are present, and also for those who have been unable to leave their daily round of duty, I desire to thank the Lieutenant-Governor for his kind thoughts, so eloquently expressed, concerning the medical profession, the Medical Congress and its work, and to thank you all for the warmth with which the toast of the Congress has been received. M. Paul Bourget, in one of his *Drames de Famille*, says that every great physician must have a direct grip of reality, an unusual power of personal yea-and-nay, of swift decision, of effective action. He compares the great physician with the great dramatic author, the great comedian. This comparison seems strange at first, but the great comedian must profoundly understand the character he would represent, appreciating how circumstance affects mood, and how mood expresses itself in speech and action, and the understanding should be informed with perfect sympathy. (Applause.) Such insight and sympathy are demanded in the daily work of the physician when ministering to mind or body in conditions of disease. The same great critic speaks of the physician as the supreme altruist, as one who is in the true way, whatever be the metaphysical postulate under which we take our stand. If there is any large degree of justification for this striking statement, it is strange to find how the community deals with the profession. Universities and medical schools are created at great cost, and curricula are devised with infinite trouble, so that the young practitioner, after many years of study, may be prepared as fully as possible for his special work. Boards of registration are provided so that those who have been adequately trained may be distinguished from those who have not. Medical associations are formed, so that the science and art of healing may be promoted, and so that high principle, kindness, and courtesy may prevail in the relations of medical men with one another and with the public. But, while the registered practitioner is thus restrained by ethical considerations, every quack, however untrained, however ignorant, is at liberty to practise for gain and reward, to blow his trumpet at every street corner, to advertise himself in all possible ways. No man without a certificate of competency is permitted to take charge of a small steam-engine, but the human body, with its marvellous intricacy, is placed at the mercy of any adventurer. (Applause.) The public has some degree of effective protection in pharmacy and dentistry, but in medicine the protection is absurdly inadequate. Until opinion is ripe for the suppression of unqualified practice, some glaring abuses might be dealt with in the spirit of recent legislation in Germany. Any advertisement of power to cure the special diseases of men or of women should be made an offence. The advertisement or exposure for sale of any means for preventing conception or procuring miscarriage should be made unlawful, the onus of proof lying on those who use the vague suggestive phrases now so common. Every bottle or packet of proprietary medicine should be inscribed with its formula, and advertisement of worthless nostrums as panaceas should constitute breach of warranty. German experience, like the experience of New Zealand, shows that this can be

done without unfair damage to patent rights, and with great advantage to the community. We would also appeal to the great Friendly Societies, whose good work we so fully recognise, in providing medical attendance for their members, to make quality their watchword, rather than cheapness, and not to open their medical benefits to the relatively well-to-do, who should in self-respect avoid the relief they do not need. In the wider field of public health, we need more whole-time inspectors, specially trained for their work, reporting to the municipal bodies and to the Boards of Public Health. The country districts should have a larger share in all that is being done for our metropolitan cities, so that pure water supply, decent drainage, and proper disposal of refuse may become universal. We urge that increased efforts be made to bring fresh air and sunlight into all places in town or country that are now confined and dark. Above all, we plead for the children, that the beginnings of life may be sheltered, fostered, strengthened by all means that wise and loving care can devise. Preventive medicine is the glory of our profession, but every man and woman should be enlisted in the army of reform; and so, in thanking you on behalf of the Congress for your kind words and your demonstration of cordiality, I would cry, through you, to the entire community—"Come over and help us." (Cheers.)

DR. JAMIESON then proposed the toast of the "Visiting Members of Congress." The Victorian members were glad to welcome the Fleet Surgeon, who had been deputed by the Admiral to represent the Naval Medical Service. They were glad to welcome a graduate of their own Medical School, who was empowered to represent the Royal Army Medical Corps in India. They offered hands of greeting to Dr. Mason and his colleagues from the Dominion of New Zealand. To their brethren from the sister States of Australia they wished to express their recognition of the self-sacrifice which had brought an unexampled number of practitioners together from all parts of the Commonwealth, and, as representing the visitors from Australia, he would mention the name of Professor Stirling, of Adelaide, who had been favoured with the Fellowship of the Royal Society of London. He trusted that the visitors would be satisfied with their reception, and with the programme of work and entertainment prepared for them; but the Victorian members were most grateful for the full response to their invitation.

FLEET-SURGEON J. FALCONER HALL responded for the Naval Medical Service. He had been greatly impressed, not only by the general efficiency of the Congress, but also, in particular, with the excellence of the work done in the Naval and Military Section.

CAPTAIN J. F. C. MACKENZIE responded for the Royal Army Medical Corps in India. It would give him the greatest pleasure to report concerning his experience at the Congress. He believed that there would be, in future, a larger communication between the Army Medical Corps in India and the Medical Congresses and Associations of Australasia.

DR. J. M. MASON responded for the visiting members from New Zealand, expressing their gratification at the warmth of the welcome they had received.

PROFESSOR STIRLING replied for the visiting members from the sister States in a speech full of gracious feeling charmingly expressed.

THE PRESIDENT then, on behalf of the Victorian Medical Women's Society, invited members to join the Ladies at the "At Home" of the Society in the Paris Café.

GENERAL MEETING OF CONGRESS.

THURSDAY, 22ND OCTOBER, 10.15 A.M.

(WILSON HALL.)

Congress next met in the Wilson Hall, on Thursday morning, when the President of the Section of Surgery, Benjamin Poulton, M.D., Melb., M.R.C.S., Eng., Lecturer on Surgery in the University of Adelaide, delivered the Address in Surgery, taking as his subject "Some Surgical Tuberculous Infections." There was a large attendance of members, and the President of the Congress, who took the Chair, expressed the thanks of the Congress to Dr. Poulton for his thoughtful and practical address, which will be found at the head of the proceedings of the Section of Surgery.

FRIDAY, 23RD OCTOBER, 2.15 P.M.

(MASONIC HALL.)

Members of Congress assembled in great numbers at the Masonic Hall, Collins-street, on Friday afternoon, for the special discussion concerning Syphilis, the President (Professor Allen) occupying the Chair. The proceedings opened with an Address by the President of the Congress, which was followed by papers or speeches by representatives of the various Sections, and subsequently by a general debate. The following is a detailed report of the proceedings:—

DISCUSSION ON SYPHILIS.

INTRODUCTORY ADDRESS.

By PROFESSOR H. B. ALLEN, M.D., President of the Congress.

The request of the Committee that I should open this debate with a short address, enables me to bring before you the conclusions that have been gradually forced upon me by experience during the past 32 years in the pathological department of the Melbourne Hospital, and in the pathological museum and laboratories of the University of Melbourne.

In the first place, the time-honoured diagnostics of syphilis leave much to be desired. The histories given by patients cannot be trusted. The common entry in case-books, "No history of Syphilis," is almost worthless. In post-mortem practice, eruptions, nodes, changes in the teeth and external lesions of the eyes are infrequent and of relatively little value. The great marks of syphilis are in the arterial walls, to which the spirochætes soon betake themselves in experimental syphilis. No other cause than syphilis can, in my opinion, account for the precocious forms of atheroma, when delicate linear opacities appear in the descending aorta at about the age of fifteen, and change from white to yellow during the

next decade, or when small abruptly prominent rounded patches arise in the ascending aorta between twenty and thirty, and soon pass into the yellow stage. Apart from atheroma præcox, which belongs mainly to inherited syphilis, all cases of severe ulcerous atheroma must be attributed to tertiary syphilis; and also, at the other extreme, the slow atrophic form with closely set wrinkling of the surface, this wrinkled atrophy being most common after middle age, but sometimes presenting itself in adolescents with inherited disease. Large thick glossy plates near the aortic valves, or wide-spread thickening of all the coats are always associated with other signs of syphilis. Marked atheroma in the arch in persons with low blood tension and small left ventricle is very suggestive. A strong tendency to relapse should be regarded as suspicious, for example, when calcified plates and atrophic wrinkles are found side by side with yellow patches of varying age, and with recent white thickenings, and possibly with ulcers, the intima being often stained by hæmorrhages and pigmentation from rupture of incoming vasa vasorum. The microscope shows how wide-spread is the thickening of the coats of the small arteries, and evidence to the same effect is given by the deep aspect of the calvarium, the larger meningeal arteries ploughing deep furrows in the bone, while the arterioles down to their smallest branches chase the surface with delicate engraving. Next in diagnostic value to the vascular phenomena, I would place a wide-spread fibrosis of organs, the smooth pliancy of liver and spleen and kidneys being very notable, implying a more or less profound alteration in general metabolism, a diminished resistance against the external causes of disease, and a tendency to untoward termination of morbid processes. In addition, careful attention should be given to the serous coats of heart, lungs, liver, spleen, &c., not merely with reference to gross thickening or adhesion, but even more particularly with regard to linear opacities or white patches with branching lines running from them. Any pathologist who will systematically record the changes in the aorta, and particularly the descending aorta, the incidence of fibrosis, the markings in the serous capsules, and particularly the coexistence of these changes, will find material for serious consideration. Using such criteria, I analyzed, in 1904, the records of a hundred consecutive autopsies performed and recorded by myself in the Melbourne Hospital; 34 showed clear signs of syphilis, 19 others showed doubtful signs, and 1 was open to suspicion (*Intercolonial Medical Journal of Australasia*, 20th September, 1904). For this Congress, I have again analysed a hundred consecutive cases, and find 32 syphilitic, and 30 more or less doubtful. There is no purposed selection of cases; but I examine only a fraction of the cases dying in hospital, a fraction not truly representative, and the hospital population itself does not represent the average of the community. Hence, my results cannot be applied to the whole body of in-patients in the hospital, and still less to the community as a whole. But, whatever weight is given to my criteria, these results are of momentous significance.

If we turn to questions interesting some of the Sections of Congress, I have before the Section of Pathology set forth some definite conclusions concerning the circulatory system. Firstly, aortic aneurism is in the great majority of cases due to syphilis, and aneurism is very prevalent in Victoria, and very markedly prevalent among women, so that in 1906 there were 31 deaths recorded from aneurism in women, as against 57 deaths in men. Secondly, aortic regurgitation is mainly due to syphilis, either

through chronic endocarditis or atheroma, or a combination of these conditions. Thirdly, a very large proportion of cases of mitral obstruction must be attributed to syphilis. Fourthly, syphilis frequently induces thickening of the fibrous rings at the bases of the valves. Fifthly, subject to unimportant exceptions, progressive chronic myocarditis is a syphilitic affection. Sixthly, syphilis is a potent cause of coronary atheroma, and thus contributes to the gravity of many cases of angina, or leads to myomalacia and rupture of the heart; while syphilitic arteritis at the mouths of the coronary arteries often causes cardiac ischæmia, claudication, and syncope.

Apart from dust disease and chronic tubercle, syphilis is the great cause of interstitial pneumonia, and it often implants a special character on the lesions of chronic tubercle. The anæmia of syphilis is well-known, and the saying that mercury is the iron of syphilis epitomizes a world of wisdom; but it is not sufficiently recognised that even the grave forms of pernicious anæmia may be essentially syphilitic. The multiple fibroses of serous membranes are often ascribed to tubercle, but, in my experience, syphilis is by far the most frequent cause. Repeated cases of pancreatic diabetes have come under my observation in which diffuse syphilitic fibrosis has been demonstrated, or a more local syphilitic process with cicatricial atrophy. Mott unhesitatingly pronounces general paralysis and tabes dorsalis to be syphilitic affections, and increasing knowledge tends to show that a very large proportion of the more obscure affections of the nervous system have parasymphilis as one important factor.

Surgeons have constantly to remember the possibilities of syphilitic arteritis and thrombosis, and not only in senile gangrene, but also in the necroses that occur in young people, and also in Raynaud's disease, which is often partly the effect of cold on tissue badly supplied by syphilitic arteries; and in thrombosis of the superior mesenteric artery with consequent hæmorrhage and peritonitis. The so-called malignant tumours that spontaneously disappear are usually syphilitic, whether in the abdominal wall or the liver or elsewhere; and, on the other hand, a chronic syphilitic inflammation is often the forerunner of true malignant growth, of sarcoma in the testis, of carcinoma in the breast. An excised ulcer of the stomach sometimes proves to be syphilitic, but a corner of it may be already carcinomatous. In the tongue and in the larynx, mistakes may easily occur in dealing with the irregular forms of syphilis and epithelioma, and syphilis may give a special character to epithelial growths. The diagnosis of gumma of the brain from glioma, and of both from gummatous conditions of the meninges, must sometimes be of extreme difficulty.

For the obstetrician, the questions to which syphilis gives rise are of profound importance. What are the signs of syphilis in the umbilical cord and placenta? In the cord, the walls of the blood vessels are thickened and sclerosed, so that after a short stay in formalin they feel like hard cords against the finger; and, in the fresh section, white fibrillar tissue can be seen spreading from the thickened vessels into the jelly of Wharton. The two arteries are constantly affected, though not always equally, but the vein may share fully in the process. The thickening may be most marked near the placenta, or at a distance from it. In the placenta itself, there may be opaque white patches in the membrane covering the free surface, and beneath these markings there may be fibrosis extending into the placental substance, perhaps surrounding some sclerosed main branches of the blood vessels. In the early stages of placental growth,

thickened vessels, local thrombosis and patchy hæmorrhage may fill the picture. White fibrosis may become general at an early stage, with typical small hard white placenta; or it may set in later and in more patchy fashion, and be complicated by the scars of old hæmorrhage. In some of the worst cases, calcareous infiltration may be in progress near the attached surface. Not infrequently, definite gummata are found, and these may undergo mucoid degeneration at their centres. What then is the duty of the obstetrician who finds such conditions? Or, apart from examination of the afterbirth, when a woman after repeated miscarriages bears a syphilitic child, what is the duty of the attendant? The mother is always infected, though she shows no outward sign of the disease. This is certain. She can suckle her child with impunity, whereas it would be criminal to give that child to a clean wet-nurse. Spironemata have been found long afterwards in the inguinal glands of such a mother; she may after a subsequent marriage bear infected children to a clean husband; she may late in life develop gummata and other typical tertiary lesions. Is it not the duty of the accoucheur at once to treat father, mother and child? It has recently been suggested that the separation of the placenta marks the time of infection of the child, and that the six weeks following birth are the time of incubation; but babies that die at birth or soon afterwards have already typical fibrosis in their organs. It has also been alleged that paternal conceptional syphilis is impossible, because the spironema is too large to be carried in the head of the spermatozoon. The neck of the spermatozoon is as important as the head. Colles' law seems to demand our belief in primary paternal infection of the ovum, and it is quite possible that the spironema is only one stage in a complicated life cycle of the parasite. It has even been suggested that the spironema is an involution form of a spore-bearing bacillus.

The gynæcologist must reckon with the rapid destruction wrought by gummata in the cervix and upper part of the vagina, and with the slower forms of ulceration and fibrosis—with forms too rapid for cancer, with forms too indolent, forms destroying the cervix and the vesicovaginal septum in a few weeks, forms persisting for months and years without corresponding alteration in general health. Chronic metritis, chronic salpingitis, chronic mastitis, in all of these syphilis may play a part, and may be the essential factor. In children with inherited syphilis, the ovaries may show pliant fibrosis and the commencement of cystic change.

From the stand-point of public health, it is distressing to contemplate the loss of life that inherited syphilis entails by miscarriage and by early marasmus. I am convinced that the main mortality of children from slight causes is due to inherited syphilis. In deaths from slight shock or slight injury, from administration of anæsthetics for some trifling condition, from mild diphtheria or typhoid or influenza, post-mortem examination reveals the typical smooth fibrosis of liver, spleen and kidneys, the increased framework of the lungs, the dilated heart, and after fifteen years of age precocious atheroma will often be added. Compare the accounts of lymphatism with the picture of inherited syphilis. Think of the white pneumonia, the cirrhosis of the liver of childhood. Then remember that a large proportion of those with minor stigmata do not die early. What is the after history of their fibroid organs, their thickened arteries, their overworked hearts? To what extent may the enormous prevalence of fibrosis of organs and sclerosis of arteries in later life be attributed at least in part to the evolution of these early changes? The problem is disquieting, the answer difficult.

Such considerations lead me to believe that the prevalent marriage law for syphilitics is not sufficiently stringent, nor administered with adequate firmness. Time will not permit me to do more than quote Marshall's recent summary of the development of expert opinion in this respect. "In 1854, Diday proposed a period of 'two years at least' without symptoms before marriage (this period of two years is still regarded by Mr. Hutchinson as sufficient). In 1890, Fournier (*Syphilis et Mariage*, 2^{me} Edn.) increased the period to 'three or four years as a minimum.' Since then the marriage of syphilitics is sanctioned after four years by Prince Morrow (1904), after five years by Ledermann (1904), and Balzer (1906). The most recent dictum from Professor Gaucher—the successor of Fournier at the Saint Louis Hospital, Paris—is five years, provided no symptoms have occurred during the last two years." It need not be added that lapse of time must be reinforced by effective and continued treatment.

In this address, I have only been able to indicate certain main lines of thought; but what I have said from the stand-point of a pathologist may serve as an introduction to the ripe experience of physicians, surgeons, and specialists in various departments. I fervently trust that this debate will materially assist in quickening observation, in settling opinion, in defining practice, in regard to a subject which seems to me of the utmost importance for the well-being of the individual, and for the safety of the community. (Loud applause.)

DR. G. E. RENNIE (Sydney), President of the Section of Medicine, said:—We discussed this matter of syphilis in the Section of Medicine on Tuesday morning, and I was instructed to bring before you some conclusions at which we arrived. Professor Allen has, in a very lucid paper, given us the subject from a pathological point of view; but, as physicians, we are not disposed to accept altogether his conclusions. (Hear, hear.) In other words, while we are prepared to admit that the pathologist has the advantage of a physician in being able to examine the organs, we, as physicians, have to examine the patients, and are not able, of course, to make a satisfactory examination in the living subject of these organs; but, so far as we are able to come to a conclusion, we are not prepared to accept the view that syphilis is such a prevalent cause of arterial degeneration. (Applause.) I think there can be no doubt that aneurism is almost always a syphilitic manifestation leading through trauma. I do not think aneurism ever occurs apart from a syphilitic condition of the artery. But when we come to consider the condition of general arterial degeneration, we are disposed to think that syphilis is not so prevalent a cause, but that other toxic agents may be at work in producing arterial degeneration. So far as regards the nervous system, we are prepared to admit that certainly in a very large proportion of cases of chronic degenerative disease of the nervous system, syphilis is a prevalent, in fact, the most prevalent, cause. Certainly in 80 to 90 per cent. of the cases of tabes and general paralysis of the insane, we may say syphilis is antecedent; and certainly in many a degeneration of the nervous system syphilis plays a like part. Syphilis is responsible for what we may call the more acute manifestations of disease of the brain and cerebral nervous system. While we are thus disposed to not quite agree with the pathologist that syphilis is so prevalent, I think we must all admit that syphilis is a most terrible disease. (Hear, hear.) In fact, I would say this, that syphilis is the most serious disease any individual can contract. In fact, with no disease ought a man, who passes his sojourn here, to be so much in fear. While we are prepared to admit that, if treated for two or three years after the initial sore has

been contracted, the patient may possibly pass the rest of his time here without any other manifestation of syphilis, he yet goes all his days in fear. That is, that at any period of his life he may develop arterial disease or grave nervous disease. At no period of his life can we say that he is free from the possibility of becoming tabetic, or generally paralytic, or of suffering from general arterial degeneration. Looked at from this point of view, we may say syphilis is an incurable disease. Under these conditions, I think that, from the point of view of treatment, it is essential that every patient who has contracted syphilis should be treated for at least two or three years in the initial stages, and not only that, but every year he should have a course of treatment, no matter whether he shows symptoms or not. I always advise any patients I come across to undergo for six to eight weeks' treatment with iodide of potassium or mercury every year. That, I believe, will possibly help to avoid the occurrence of subsequent manifestations, but as for the para-syphilitic manifestations—that is, tabes, general paralysis, and so on—when once these have occurred, then anti-syphilitic remedies are useless. Under these conditions, I repeat that while we, as physicians, are not disposed to accept the dictum that syphilis is so very prevalent, we are still of opinion that syphilis is a very terrible disease. (Applause.)

MR. F. D. BIRD (Melbourne) said that there was no distinct discussion on syphilis in the Section of Surgery at this Congress, so that he would venture to place before the meeting a few remarks which he had prepared to occupy the ten minutes allotted to him:—

The subject of syphilis concerns surgery generally in large degrees, and its local manifestations touch surgery at many points. The unknown quantity in surgical diagnosis and also in prognosis is often made up in a considerable measure by syphilis. This more or less hidden influence of syphilis has imprinted itself on the minds of surgeons as mysterious and baneful for very long in the history of surgery, but since the discovery of the *treponema pallidum* as the cause of the disease, while the banefulness has by no means diminished, the mystery has, and we see more clearly many things, *e.g.*, why syphilis may remain latent so long, why early manifestations are so likely to communicate the disease by contagion, and why later lesions lack this faculty so largely.

It is because the *treponemata* and their toxins may affect normal development in greater or less degree, and in various ways, that syphilis concerns the surgeon in a general regard. The toxic influence acting upon the growth of the individual may have left its mark in a universal want of robustness, which is at once obvious to the surgeon's eye, and which is often in salient contrast with the evident sturdiness of the parents. That is a very important point, for you will often see fine upstanding parents having children obviously not of the same type as themselves. This, to my mind, betokens syphilitic poison at work. The blood, a world in itself, shares in the poisoning, and a marked anæmia adds to an appearance already indicative of a weak hold upon life. Such a patient the surgeon feels instinctively is not a fair subject for prolonged operation.

Unfortunately, however, from an operative point of view, the fact of the individual's development having taken place under the shadow of the syphilitic poison is not always so apparent, and the still present weakness is masked by a framework formed on generous lines, an anæmia alone showing that some toxic influence has been at work. I can name many cases in my own practice where people have nearly died in operations under chloroform, who were made on big massive lines, and yet they were undoubtedly syphilitic, as I discovered afterwards in a variety of ways. Such cases bear surgical work badly. A poor blood is in them, I believe, often associated with a poor centre of circulation. They show signs of shock out of proportion to the surgical causes of it acting in their particular case. These instances furnish a fair proportion of the deaths which occur from chloroform administration. If I know or suspect a case to be congenitally syphilitic, a warning is always given by me to the anesthetist of the possibility of the presence of a heart developed under the disability of the syphilitic toxin.

Have we any means of judging whether in any given case a heart is present which will not stand a reasonable amount of anæsthesia and shock?

Some of these hearts are obviously small and ill-developed; others, again, are of a fair size but a poor quality, and they show with the fluorescent screen a feeble shadow in the cardiac area. Whether all do I cannot say, but I know that I have observed this want of density in some and want of size in others in the heart shadow in congenital syphilitics, a sufficient number of times to make me attach much importance to it. I have "pulled the leg" of my X-ray man on a number of occasions by telling him "we shall see in this case a poor heart shadow." Perhaps I knew the parentage was syphilitic, or I had other reason to believe that the person was a congenital syphilitic; and true enough, on the fluorescent screen a very poor heart shadow has been seen, quite out of proportion to the size of the patient. A fair percentage of people who die easily, who succumb to apparently inadequate causes, are congenital syphilitics; and it is of moment to the surgeon to be able to recognise them. I believe the fluorescent screen affords him some help in the direction just spoken of. Other assistance may be given by the X-rays. In the adult, as a general factor, the syphilitic poison displays itself chemically also as a producer of anæmia, and, although a middle-aged man's anæmia suggests tertiary syphilis, it also suggests carcinoma, and here a difficulty in diagnosis arises, not only viewing the case as a whole, but in deciding on the nature of individual manifestations.

There can be no doubt that cases of tertiary syphilis easily relievably by treatment are not infrequently diagnosed as carcinoma, and accordingly left without the drugs which could work such wonderful amelioration. This error in diagnosis is mainly likely to occur in regard to visceral syphilis; but lesions of visual accessibility are at times so like cancerous growth and ulcerations that the mistake is easily made. Thus the surgeon who believes, as I do, that syphilis is a very common disease ought to keep the possibility of it always in his mind, ought to look upon syphilis as an ever-hanging cloud upon the horizon of his diagnosis. He who looks out for syphilis will always have plenty to engage his attention and to direct his treatment in anomalous cases. Syphilis will occasionally simulate pernicious anæmia, and an unexplained loss of health and colour in a middle-aged person should suggest syphilitic possibilities. Such chronic devitalization is usually seen in married women, whose syphilis-giving husbands have suffered as yet but little from their disease, and who show no signs of it when the wife seeks advice. These cases generally suffer from some menstrual aberration which is considered but part of the general ill-health, though sometimes worthy of a curettage. The submission of the curette debris to the microscope will often start or add to our suspicions of syphilis, and a course of treatment restores the patient to health. The relation of syphilis to nephritis in causality is a different one. My mental attitude to the question is that some few in a hundred nephritic cases are undoubtedly of syphilitic origin, that a large percentage are equally undoubtedly not of syphilitic origin, and that the rest, a considerable number, may be due to the treponema or its toxins; but it is impossible to be sure, although the test of the deviation of the complement may render the probability a high one. Those who ascribe most of nephritis to syphilis are as far from the truth, in my opinion, as those who think it a rarity when of syphilitic origin. Considering how much the kidney has to put up with after many operations under general anæsthesia, its valency must be of high moment to the surgeon, and the X. of syphilis may play a great though hidden part in disturbing its structure.

There are two symptoms, the frequency of whose dependence on syphilis is not, I believe, estimated at a sufficiently high figure.

Unexplained headache in a young subject and sleeplessness in a middle-aged man are to me symptoms strongly suggestive of syphilis.

When we come to deal with local evidences of syphilis, we see that the treponema has an equal right with the grandiloquent Roman to ask, "*Quæ regio non plena nostri laboris?*" The evidence of syphilis may turn up anywhere. Thus the surgeon is never safe, in the diagnosis of any local lesion, to allow no admission to syphilitic possibilities. Many ulcers have no treponema to maintain them as ulcers; but, surely, no consideration of the nature of any given ulcer is complete without thinking of syphilis, when we remember how marked a feature ulceration is in the natural history of the disease, from its initiation into the human body to its last manifestations in middle or old age, from cranial ulceration at the top to paronychia at the extremity. Thus our diagnostic efforts are not complete in any case where ulcer exists in the stomach or in the rectum, in the mouth or at the arms, in the region

of the knee or around the finger nail, if they do not consider the possible presence of the treponema. Likewise with fibroid thickenings in various places, we all, of course, admit that many other toxines may cause fibrosis of various structures; but when the reaction of the human organism to syphilis is so much a matter of fibrosis on account of its chronicity, can we safely omit syphilis from our list of possible causes in any given case. It is here that in the viscera we find such difficulty in deciding which toxine has caused the reaction resulting in the fibrosis in the case under consideration, and a stale-mate results. Thus, with a liver fibrosis, in many examples nobody can say definitely it is syphilis, and nobody can say definitely that it is not, but all may agree in admitting that it may be syphilis. Thus any fibrosis anywhere may be syphilitic, and therefore this disease must enter the diagnostic area whenever fibrotic conditions are considered. While the relation of these two conditions to syphilis is well known, it is by no means always considered when these conditions are present, and certainly in cases of ulcer or fibrosis the possibility of syphilis ought to come to one's mind.

Not so well known is the power of syphilis to form definite discrete tumours, which do not impress the examiner at all as infiltrations. These are, I am sure, a fertile cause of error, and may be found in many places. One of the most interesting features in my practice during the last few years has been to recognise lumps in different parts of the body as syphilitic, where at first I never thought of syphilis, and finding that syphilis could produce ulcers on the forearm and other parts of the body, these lumps and ulcers being undoubtedly syphilitic gummata in various stages of fibrosis. An additional three years' experience has not impaired in my mind the correctness of the views enunciated by me at the last Congress on the subject of abdominal syphilis. It has, on the contrary, strengthened my previous observations, and it is a source of much gratification to me to have received at this Congress a number of statements from members to the effect of their having since last Congress become aware in their own practices of these phenomena and seized of their syphilitic origin. Though most of my observational knowledge of syphilis has been collected from those who have acquired the disease, I am as definitely impressed with its commonness and its capability of explaining anomalous conditions in those who have their teeth set on edge because their fathers have eaten the sour grapes of syphilis.

DR. FLASHMAN (Sydney).—I do not propose, after the very exhaustive paper that the President of the Congress has given us, to deal very much with the pathological questions affecting syphilis. We discussed the question of syphilis in the Neurological Section, and we are in agreement with all the other Sections. We view syphilis as one of the greatest diseases which we, as medical men, have to combat. Dr. Rennie, in reporting from the Section of Medicine, said that they did not quite agree with the views expressed by Professor Allen; but, in listening to Dr. Rennie, I am sure that we can feel that it was like chipping away a little bit of the Great Pyramid with a needle. After saying that they did not agree with the views expressed by Professor Allen, he enumerated a long list of diseases which they were entirely agreed were due to syphilis. (Hear, hear.) The question is not whether 50 per cent., 60 per cent., or 70 per cent. of our ills are due to syphilis, but whether a large proportion of the ills that we suffer from are due to syphilis. If we are agreed upon that, and I think we are all agreed upon it, then I say it is our bounden duty to take some very definite steps to do the best in our power to combat the evil. I, in the Section of Neurology, was opposed to the Section coming to any definite conclusion as to what steps should be taken. The Section carried the proposal that it should be a suggestion from the Section to the Congress to pass a resolution appointing a Committee, more or less permanent. I have the resolution, which has been intrusted to me by the Secretary of the Neurological Section, proposing that a Committee of this Congress be appointed, and I would urge with all my power that this Congress should not break up without taking some step which will lead to something definite. We may meet here and discuss all the afternoon the various regions of syphilis, but we have a great opportunity now,

being met here together in Melbourne, of taking some definite step which may lead to a diminution of this evil. We are all trying to combat it individually. Now is our opportunity to take some collective action. (Cheers.) I will read the resolution which the Section came to, and I will leave it to the meeting, placing it in the hands of the President for any action that the Congress may deem necessary. The motion we agreed upon is this—"That this Section recommends the General Congress to appoint a Committee to investigate, tabulate, and report the facts in regard to syphilis which such Committee may obtain: and that the Committee report to the next meeting of Congress, and be given authority to publish its report prior to the meeting of Congress if its investigation convinces the members of the Committee of the necessity for such urgent action." I beg to support that motion, and to point out that there is a tremendous number of questions connected with the subject of syphilis. Such a Committee, I propose, should consist of men who would take great interest in the pathological side. We all know that side has made great advances in recent years, especially in regard to the serum test for syphilis in the deviation of the complement. That this Committee consist of men who would be interested in that phase of the question, and also of those who would be interested in observing the various sociological measures that have been adopted or recommended in various countries, and to observe the effects such measures have had, so that we would be in a position to publish for the use of all Australasia, and also for the older world, the facts tabulated in a proper way, putting before us all the most recent work in regard to syphilis. (Cheers.)

Dr. MASON (New Zealand), President of the Section of Public Health, said,—I am very thankful that the form of resolution sent on from the Section of Public Health does not require me to say whether syphilis is prevalent, as one side alleges, or whether the cases are so few as the other side say. The point of view we took up was, what could be done by the State in order to lessen what we must all admit is the presence of a very formidable disease. (Hear, hear.) Medical men who have been in the habit of advocating the introduction of, or the continuance of, Acts such as the Contagious Diseases Act, are usually assailed by the women folk, and I think the women are making the very greatest mistake they can in suggesting that the medical man is speaking as a man, and not as a conservator of the public health. (Hear, hear.) To whom does the woman in heavy sorrow go when in this trouble? A meeting of women? No! Why, they will often have very little to do with it. To the medical man, who is accused by the women of acting unfairly towards women in these matters? To him does the poor woman in trouble go. I wish to speak quite definitely, and I am authorized by our Section to so speak, that any restriction by legislative enactment that may be brought as a consequence of this meeting is as much in the interests of the women as it is in the interests of the men. (Hear, hear.) Now you have all sorts of regulations in all sorts of different countries. Personally, I do not believe that regulation is of very much effect at all. Quite recently, when in Paris, I was asked by our Government to inquire into these matters, and the French Government very kindly gave me every opportunity of looking into the methods adopted, and in a measure assessing the value of them. It has been pointed out, not once, but many times, that the recognised prostitute is not the one that gives syphilis in the main, because 79 per cent. of the women who have given syphilis to the male were not inside the house of correction at all. (Hear, hear.) So you see the safety which

the women and some opponents of registration allege will not come from registration as a matter of fact does not. The woman in the house has usually been so syphilised that she is practically unable to give a client anything at all. She has had it so thoroughly. And most of the authorities in Paris assured me that the danger was not in the houses, where the women were few, comparatively speaking, but outside, where the applicants for employment were many. There is one other point that I would like to dwell upon, which is not quite covered by a resolution adopted by our Section. We must admit that prostitution does exist, and when a person elects—whatever means has brought the person to that point of election has nothing to do with us sanitarians; that is a matter for the moralist—but when a person elects to ply a trade, the consequences of which are such as have been detailed by our distinguished President, we hold that that person should be controlled, just the same as any other person plying a noxious trade. (Hear, hear.) But some of us go further than most advocates of restriction go, and we say that “any male seen frequenting the vicinity or the house of an acknowledged prostitute, if he frequents that place with such frequency as to justify a Court in adjudicating him a habitual frequenter, he should go on the list, too.” (Laughter, and cheers.) One of the resolutions which we carried was, “That in the opinion of the Section of Public Health and State Medicine, syphilis should be placed on the list of notifiable diseases”—(that, I have very little hope of seeing carried in any particular)—and “that suitable hospital accommodation should be provided for the treatment of cases of this disease.” Now, speaking of that first resolution, there is to my mind no just reason why syphilis should not be put upon the list of notifiable diseases, just the same as other infectious diseases are. (Hear, hear, and “No.”) A gentleman says “No.” Possibly he will have an opportunity for giving a reason. But why, because a disease is acquired in a specially private and intimate manner, should it be excluded from the list? If it is doing the harm which it is alleged to be doing, we, who are responsible for the general health of the community, should surely be seized of the size of the enemy which we have to deal with, and the first step towards that is to put syphilis, just as consumption is, upon the list of notifiable diseases. (Hear, hear.) I have advocated this for many years. I have not yet been successful in getting it on. Many of the arguments offered against it were offered against the inclusion of tuberculosis on the list of notifiable diseases. It was urged that it would be a breach of confidence. There could be no breach of confidence when the law required you to do it. (Cries of “Yes.”) There may, but so far as the medical practitioner and his client is concerned, he is exonerated from all blame, and equally from actions. (A Delegate.—“Legal blame.”) Legal blame is entirely what I am dealing with. That being so, we do not expect that the same amount of notification or particulars should be supplied to the health officials as in the case of scarlet fever, but we hold that the first step towards getting hold of the enemy is to see exactly how big he is. The second resolution passed at our Section reads as follows:—“That legislation be enacted to insure that persons from oversea suffering from active infective syphilis should be liable to quarantine restriction.” (A Delegate.—“How long? Three or five years?”) (Laughter.) I can see the audience is not entirely with me in this, and I am quite prepared to hear some one say it is unworkable and out of the question. As soon as any person says so, I say it is the law and the practice in New Zealand. (Hear, hear.) No person suffering from syphilis is allowed to land in the Dominion of New Zealand.

(Cheers.) I do not say for a moment that any one suffering from that disease does not land. (Laughter.) The object of this resolution is to give the same power to the Australian States as we have in New Zealand and in many of the States of America. There is only the one other point, an important point, which was raised in our Section, and that is—does it come within the role of the medical practitioner, looking to the fact that syphilis is rife, acknowledging as we must that prostitution does exist, whether we should go as far as they do in some countries and offer the individual some means of protecting himself against inoculation. As most of you know, in Germany, in France, and in every other navy except our own and that of America, I believe clear instructions are issued to all men, since Metchnikoff and Borrel have shown that an inunction of oleate of mercury is in most instances a complete remover of any danger. It is a matter for careful consideration whether we should not tell possible acquirers of the disease how best to prevent it. (Long and loud cheers.)

DR. LOCKHART GIBSON (Brisbane), President of the Section¹ for Diseases of the Eye, Ear and Throat, said,—In case I forget, there is only one objection to the first proposal of Dr. Mason. That is that it would have a deterring effect on patients coming to consult medical practitioners, and anything that would deter syphilitic patients consulting a competent medical practitioner would lead to increased ravages of the disease. (Hear, hear.) We discussed this matter in the Eye Section. No resolutions were passed, but I was intrusted to put what I took to be our views before this meeting. And if the views of the Section are not correctly interpreted by me, they must be taken as my views, and not necessarily of the Section. As far as possible, I shall try to put the views of the Section before the meeting. I think there are very few eye practitioners who are not prepared to accept Professor Allen's conclusions. (Hear, hear.) And also prepared to accept the conclusions of Dr. Bird. No paper published in Australia has impressed me more than the paper of Dr. Bird at the Adelaide Congress on Abdominal Syphilis. (Hear, hear.) I cannot say I am altogether surprised to hear the conclusions of physicians in this matter. I think it will be the experience of most eye practitioners that physicians are not as fully alive to the prevalence of syphilis as they will be in the future. (Hear, hear.) I think physicians are too ready to accept the histories of patients, and to judge from the characters of patients, and, therefore, to assume that there has been no opportunity for contracting the disease. (A Delegate.—“Question.”) The conclusions I put I am justified in putting before the meeting as the conclusions of the Eye and Ear Section. I have written them down shortly, because I think if I put them in that way I will take up less time of the meeting.

1. That the different tissues of the eye are frequently affected as a result of inherited or acquired syphilis, and that a considerable percentage of the cases seen by eye practitioners are suffering from that disease. If we exclude refraction cases, the percentage is very considerable.

2. That practitioners engaged in eye work have opportunities of diagnosing manifestations of syphilis which are not possible by those engaged in other departments of medicine, because manifestations in the tissues of the eye are more often than not unaccompanied by manifestations in other parts of the body.

3. That the transparent tissues of the eye and its peculiar function gives us opportunities of observing the effects of treatment and its results more definitely than is often possible in other departments of medicine, except, perhaps, surgery. That is the only point we could possibly quarrel with in Professor Allen's paper, that his evidence could not be supported by treatment. I mean, of course, not in the cases he produced.

4. That some of the conditions found in the eye are early manifestations of the disease and very amenable to treatment.

5. That if such early manifestations lead to active and prolonged constitutional treatment, there is a fair likelihood that the patient may suffer in later life from no symptoms of the disease.

6. That if these are disregarded or imperfectly treated, not only may blindness result, but serious and possibly uncontrollable disease, for example, of the central nervous system, is sure to appear in later life.

7. That syphilis contracted in early life may be diagnosed at times, even after the age of fifty or sixty years, on account of affections of the lining, coats, media, or musculature of the eye, although no symptoms attributable to the disease have been previously noted, and the patient has been quite unaware that he had any primary affection.

8. That when the manifestation is a tertiary one, active and prolonged treatment may still lead to a comparative cure of the eye trouble, and at the same time lead to a disappearance of obscure general symptoms of ill-health.

9. That eye practitioners are somewhat handicapped by an attitude upon the part of practitioners who send cases to them of what may almost be called amused tolerance, when they are told again and again that an eye condition suggests syphilitic taint, inherited or acquired, and that anti-syphilitic treatment should be employed. This attitude upon the part of practitioners leads to a half-hearted prosecution of treatment, and, therefore, often to failure to obtain results.

10. That manifestations of inherited syphilis may be observed in the youngest infants, or may not present themselves until even thirty years of age.

11. That manifestations of acquired syphilis may be observed at any age.

12. That mercury is the drug to be chiefly relied upon, and that, when active treatment is required, inunction is the best method of administration.

13. That iodine internally may at times be administered with advantage, while mercury is being given by inunction, and that when iodine alone is relied upon the doses should be massive.

14. That the habit of placing reliance on the history of a patient is a pernicious one, and leads frequently to failure to thoroughly prosecute anti-syphilitic treatment.

15. That it is often wisest to abstain from inquiring into history, and always wise to disregard a negative history in the presence of manifestations attributable to syphilis.

16. That manifestations of syphilis are less common in the throat and ear than in the eye, and are often less amenable to treatment than in the case of the eye. That in some cases ear affections due to inherited syphilis could have been prevented if manifestation in the eyes many years earlier had led to thorough and prolonged constitutional treatment.

Dr. Lockhart Gibson concluded by saying that he had listened with great pleasure and profit to the remarks of those others who had spoken. (Applause.)

DR. McMURRAY (Sydney), President of the Section for Diseases of the Skin, said:—There was no special discussion on syphilis in our Section, but, in compliance with the request of the Committee, I have prepared a few notes, which I will place before you. (*Reads*):—

There is no doubt that venereal disease is an increasing menace to the health of the nation. According to French, in the whole army in 1903, there was an average number of 3,554 men constantly in hospital on account of venereal disease out of a total strength of 242,182 men, and in this number India contributed 1,568 out of a strength of 69,613.

One-third of the soldiers invalided home to Netley suffer from the effects of venereal disease.

According to the statistical report of the Navy for the year 1906, the Australian station tops the list.

I here quote from Table IV. showing the ratio per 1,000 of force of numbers of rates of disease on the different stations for the year 1906:—

Home, 138.13; Channel Fleet, 81.15; Atlantic, 110.91; Mediterranean, 97.84; North American, 107.3; China, 170.8; East Indies, 138.74; Cape of Good Hope, 68.04; Australian, 214.75 (*sensation*); irregular list, 130.66.

The case ratio under this heading was 214.75 per 1,000, an increase of 58.57 as compared with last year, and of 56.75 in comparison with the average ratio for past nine years. Sydney is mentioned by nearly all the medical officers as the place where these diseases are most prevalent, and during the year inquiries were set on foot to devise some means to check this pest in port. It is worthy to note that Brisbane, where a modified Contagious Diseases Act is in force, is noted as practically free from these diseases (hear, hear), and the Medical Officer of the *Pegasus* reports that at Noumea, where the French authorities rigidly enforce a Contagious Disease Act not a single case was contracted, although leave was freely given to the crew during all days of the visit of the ship to this port.

Prostitution is the oldest profession. (Hear, hear, and laughter.) It always has existed and will exist in spite of the terrible penalties resorted to for its suppression. My experience is that disease is contracted five times out of six from clandestine prostitution, and especially from young women. In them the disease is active, and virus virulent. Every one knows one is much more liable to contract disease from a young than an old prostitute. The young do not know how to keep themselves clean, and are very often unaware that anything is wrong with them. As a rule, when they have become infected, they try to conceal it as long as possible, until they are obliged to seek advice. It is looked upon as a shameful complaint. On this account many of the hospitals do not take this class of case in. The lay governors are as a rule to blame for this state of affairs. A writer has said of the hospital attitude, "We cannot receive you when your disease is acute and curable, but when your gonorrhœa has developed into stricture, salpingitis, peritonitis, or when your syphilis has affected important central organs, the brain, spinal cord, the organs of special sense, you may be received, but your disease shall be baptized under another name, which does not offend the refined susceptibilities of your patrons." (Cheers.)

I think one of the first and most important means that should be adopted to decrease syphilis is to increase hospital facilities for its treatment. Patients should be admitted without fuss or formality. Their stay in hospital should be made as attractive as possible (laughter), and above all, the patients should be impressed with the enthusiasm of all those concerned for their welfare and restoration to health. The wards for the treatment of venereal diseases should not be designated by any distinctive or opprobrious name. (Hear, hear.)

Further efforts should be made to reach those affected by gratuitous consultations and by dispensaries controlled by Government and municipal medical officers, established in convenient centres, open all night.

Venereal services should be open to all medical students, and no student should be permitted to graduate without a certificate for a six months' attendance at a venereal clinic. (Hear, hear.)

I would advocate that well-conducted brothels should not be interfered with by the police so long as they were well conducted. (Laughter.) Men entering these houses should be examined by the matron, so that all who are diseased should be eliminated. The girls living in these houses should be subjected to sanitary visits, and the elements of sexual hygiene and venereal prophylaxis inculcated verbally or by instruction card.

As Fournier has said, "To cleanse the brothel is not only to protect those who frequent it; it is also to protect the family hearth, the honest woman, the child, and the race."

DR. P. B. BENNIE (Melbourne) said:—I had the honour of reading a paper before the Section for the Diseases of Children of this Congress, and a much greater honour of being appointed their representative to speak at this meeting. I suppose they wanted me to give you something of what was in my paper, so I have prepared a brief extract, which I will read to you. Before passing judgment on what is stated here, I wish you would refer to the original paper, where you will get the data on which these inferences have been founded. They are inferences which have been drawn from the time when I commenced a practice 25 and more years ago. (*Reads.*)

Syphilitic infection is not recognised by many practitioners except when it presents itself in the strong light of the typical text-book description. (Hear, hear.) I have inferred that fully 25 per cent. of the sick children in Melbourne are tainted with syphilis, and that about 10 per cent. of the total number of

children in Melbourne are syphilized. The syphilitic factor is present in more than half the work of the paediatrist, and nearly half of the children who die are infected with syphilis.

Speaking generally, the chances of dying before puberty are for the syphilitic seven times greater than for the non-syphilitic. Syphilitics should be treated during their whole lives, and the treatment extended to the next generation. (Laughter.)

In justice to humanity syphilitics should never marry. (Hear, hear.)

The resistance to syphilis is lowered by the presence of other diseases, and the resistance to other diseases is lowered by the presence of syphilis. It is this reciprocal action which accounts in a great measure for the extreme frequency of disease and death in the children of infected families. In my cases of tubercular meningitis 60 per cent. were syphilitics; in other tubercular diseases of childhood the percentage was 30 per cent.

From my practice I have inferred that the chances of getting typhoid, measles, and scarlatina are for a syphilitic two or three times greater than for one free from infection—that is not referring to widespread epidemics, but to general practice extending over 25 years—while for diphtheria the chances are nearly seven times greater for the infected than for the non-infected. The child born of a syphilitic parent is never free from infection. He may have an outside apparently very good but a very bad inside (laughter), which is often discovered only *post mortem*. (Hear, hear.) This apparent health is the point to which particular attention should be drawn. Lowered resistance to disease is shown in the nervous system particularly, also in the circulatory, respiratory, digestive systems, and is marked in the osseous system. Post-nasals are often syphilitic, and those recurring after operation nearly always are so. Hare-lip and cleft palate cases frequently require anti-syphilitic treatment before operating.

Infected children should, if possible, be treated in all their illnesses for the syphilitic factor—the favourer of pathogenic microbe associations. If this were done, there would be fewer severe cases and fewer deaths.

Before operating on congenital syphilitics the surgeon should, if possible, subject each of them to a course of anti-syphilitic treatment, for otherwise, on account of their lowered resistance, they take anæsthetics badly, do not stand operations well, and are liable to post-operative development of infective organisms *ab interno* as well as *ab externo*. Operations would then be fewer, often more successful, and less often fatal. (Applause.)

In conclusion, Dr. Bennie submitted the following recommendation from the Section for Diseases of Children:—

A PROPOSAL TO DIMINISH THE PREVALENCE OF SYPHILIS AND OTHER VENEREAL DISEASES.

Recommended by the Section of Diseases of Children.

Syphilis is contracted mostly in youth or early manhood. It is contracted by those who are ignorant of the nature and extent of the risk they may run. They do not know how widespread the contagion is and how great the chances of infection to any one who indulges in illicit intercourse. They are ignorant of the terrible and far-reaching results of infection, of the remote vascular, visceral, and nervous degenerations. They are ignorant of its ravages in the home, and of how it destroys, cripples, or debilitates the second generation.

There is a belief or prejudice, rather widespread among young men, and, unfortunately, encouraged by some of riper years, that incontinence is, if not a necessity, at least an advantage to physical vigour, and that its contrary is a mark of weakness of body or poorness of spirit.

From this ignorance and this delusion are largely derived an immense amount of suffering and disease only too well known to the medical profession. The responsibility of those who know, to enlighten those who do not know, and are in danger, is great, pressing, and not to be evaded.

It is proposed that a committee be appointed to draw up a short statement, carefully and judiciously worded, freed from all taint of sentimentality, of the plain and honest truth on these matters for distribution among young men, this statement to be of an authoritative character and signed by leaders of the profession.

That this statement be printed as a circular and in such numbers as may be found necessary, and that the method of its distribution be determined by the Committee.

That the expenses of printing and distribution be defrayed by voluntary subscriptions from members of the medical profession, and that the A.M.G. and I.M.J. be asked to invite and receive such subscriptions.

That the Committee take immediate action, watch the effect of such action, and report on the matter to the next Session of Congress.

DR. BENNIE, referring to this recommendation, said:—I think this is the best way of meeting the difficulty. All legislation on this matter has proved to be useless in Paris. It has been tried for 100 years, and Paris is no better off than we are here. ("Question.") As regards the amount of syphilis in the adult population, in the families I have attended, it comes out at about 14 per cent, that is in promiscuous families of all sorts. I have taken 300 families which I have attended regularly for fourteen years, and 14 per cent. are infected. That is about the same as in Paris and New York; so that if we want to meet the difficulty, I do not think legislation will do any good, but we must increase cortical cerebral resistance to syphilis by educating the young. (Applause.)

DR. MAKINS (Warrnambool).—I would like to ask what are we to do with the people—I am speaking more or less as a country practitioner—people who come to us and ask us if they should marry or not? Dr. Bennie is the only man I have heard say anything, and he says definitely that if a man has once had syphilis he should not marry. I am not speaking with authority, but I am here wanting to know. A man came to me the other day, who in 1903 had a hard sore, and had been under treatment continuously since then. He has never had any other sign, no secondary symptom, nor anything else, but he had a definite hard sore diagnosed by a man in London. He wrote me the other day that he wanted to get married, and asked my advice. I have also had other patients, not quite so long after infection or with quite such good health; but what I want to know from the leaders of the profession is whether one is justified in condemning that man to perpetual celibacy or not? ("No.") Personally, I think not. Dr. Bennie says "Yes." That is all I wanted to know.

DR. J. WEBSTER (Launceston).—I think those of you who have had a fair amount of general practice will agree with me in this respect, that to alcohol alone is due a vast amount of syphilis that otherwise would not exist. I think that all who have had large experience—I have had 27 years of hospital and private practice—will agree with me that a vast number of men who never think of prostitution will get intoxicated at times perhaps at some rejoicing, and in their helpless condition on their way home are seized upon, and the disease is given, and we medical practitioners are horrified to find it in quite respectable families. We find it given to wives and children, and by men who we know from years of experience would not think of being unfaithful to their wives, if it were not for intoxication. I think a large amount of the syphilis we meet with in general practice is given to the family that way. The general public must not think that members of that family have deliberately gone to prostitutes and brothels and contracted it. Therefore, I think direct solicitation, and particularly solicitation of intoxicated men, should be prevented by law. (Cheers.)

DR. E. S. JACKSON (Brisbane).—I thought perhaps my experience may be of some interest to this meeting, because I think I am probably unique in this respect, in that I have had the management of the Lock Hospital under the Contagious Diseases Act in Queensland. Thank God it is many years ago, as it was a very unpleasant job. For six years I had charge of the Lock Hospital in Brisbane, under the Contagious Diseases Act,

which is still in force there. I have often heard it said, statistics show that the Contagious Diseases Act does not do any good in the lessening of syphilis. I have answered that by saying that if a man would go into the Lock Hospital and see the cases there of syphilis, and then go out and say those cases are not better locked up, I do not know where his intelligence can be. It is quite true that in every city where the Contagious Diseases Act is actively enforced, there are many prostitutes who are not brought under that Act by the police for many reasons; and that those prostitutes disseminate contagion in a very much greater degree than prostitutes found in the Lock Hospital. Therefore, when a man says that it is from the outside prostitutes that syphilis comes, it is merely proof that the Contagious Diseases Act is not properly worked. I have heard some things said which are of great interest to me. Professor Allen said that the frequency of aneurism in women in Melbourne was very much greater than one would have supposed. Now, we do not see very much aneurism in Brisbane, either in men or women. I think the Brisbane practitioners will bear me out in saying it is not a common disease in Brisbane. (Hear, hear!) It certainly was in existence at one time. Is it possible that our Contagious Diseases Act has protected us from aneurism? (Hear, hear!) I suppose the greater frequency of aneurism in Victoria in women than in men——

THE PRESIDENT.—No, not greater frequency. There were thirty-one deaths from aneurism in women in 1906, and fifty-seven in men.

DR. JACKSON.—My last visit to Melbourne was about eighteen months ago. Then it appeared to me that Collins-street at night was full of prostitutes from one end to the other. (Hear, hear!) Now, that is a great contrast with what we see in Brisbane. It is the rarest thing in the world, in my experience at any rate, to be accosted by prostitutes in the streets of Brisbane. Therefore, I would say that it is quite possible that a Contagious Diseases Act may deter and prevent solicitation. I have always thought so. I thought so when I was in charge of the Lock Hospital. I believe it acts in this way, that the women who are not on the list are afraid to solicit lest they should be arrested. Therefore, solicitation in the streets is lessened by a Contagious Diseases Act, as it is worked in Brisbane, at any rate. (Hear, hear!)

DR. JEFFERIS TURNER (Brisbane), President of the Section for Diseases of Children, said: It must not be thought that syphilis is a rare disease in Brisbane. Dr. Lockhart Gibson has told you that you must not go away with that impression, and further, it is a more common disease than we know. That is a thing that I have learned down in Melbourne, if I have learned anything. You will see more syphilis when you are looking out for syphilis. (Hear, hear!) In Melbourne, they look out for syphilis much more than we have done in Brisbane, and perhaps in some other parts. I do not say we have as much in Brisbane as here, and cannot say. If we have not, I cannot say why, but I know this, that in the smaller towns in Queensland, where there is no Contagious Diseases Act, from what I hear, there is much less venereal disease than in Brisbane. I think the size of the town is one factor. But apart from that, I have another remark to make, and that is that you cannot eliminate syphilis by cleansing the brothels. I am surprised to hear any one getting up and suggesting such a thing. Metchnikoff has told you that is impossible. It has been tried in Paris for 100 years, and Dr. Bennie, who staggered me by his statement as to the amount of syphilis in Melbourne, told me the

same morning that in Paris, syphilis is as common—I think he said more common—as in Melbourne. That is, after 100 years of this system, worked out more rigorously and thoroughly than you could get it worked out in a British community. I do not want to say anything more controversial, but I want to say something about this proposal that Dr. Bennie has brought forward from the Children's Section, because I think it is a proposal which we might all agree upon. I have spoken about it to several men, whose experience I had no means of discovering beforehand, and the worst I have heard against it is that it won't do much good. To that I would simply reply, "It may do some good. Why not give us a chance to try it?" I may say more than that, it will do some good. I have heard some instances—you must all know of instances—in which syphilis has been contracted in ignorance—perfect ignorance—of the consequences, and in which a few timely words of warning would have saved that man from life-long incubus; an incubus lasting two generations. Now, sir, this is a grave problem, at the gravity of which all thoughtful medical men in this meeting must be appalled. We cannot divest ourselves of responsibility, and it seems to me a plain duty that we who know the gravity of the danger should warn the great bulk of mankind, that is, all outside the medical profession, that there is the danger, and how great it is. We should warn those particularly who are exposed to this risk, especially the younger men of the community. The warning can do them no harm, and it will certainly save some, and we cannot avoid giving that warning without taking upon ourselves the responsibility for the consequences of our failure in our duty.

DR. CROWTHER (Hobart).—I should not perhaps have come on the platform, but I have felt the figures are so startling that I should certainly challenge them on behalf of the State of Tasmania. I have been through as much general practice as most men, and in a practice which has run through the third generation of the same name. I say positively that there is no such amount of syphilis in the State of Tasmania as alleged to be here. I think it is a very serious statement to make, and should be taken with great care. Unless you have some different methods with regard to death registration, by which you can get syphilis returned in some form or another, you do not know where to make a beginning. Then there is another important thing. One gentleman made a very great point with regard to examination of syphilitics prior to entrance into the different States of Australasia. Why, it is bunkum. (Laughter.) But I was told by one of my own people, who had relations in the Peninsular War, that there was one opera dancer that crippled more officers than some of the campaigns in Italy. When she was examined by a Board, there was not one trace of disease that they could prove, and yet that woman crippled them by the dozen. What is the good of an examination? If you, gentlemen, get the Contagious Diseases Act extended to any degree, or carried out beyond what it is at present, if you get that, your heads will never ache again. You have given women a vote throughout Australia except in Victoria—and that is coming—and I do not care how you talk to them or reason with them, they consider it is an indignity put upon a woman, and they won't accept it.

DR. ANDREW (Hawthorn).—In reference to this question, I think one is reminded of that question asked by a philosopher of old, "Where is truth?" Or, as the scholar put it, "What is truth?" Speaking as a general practitioner, and one who has had a lot of experience the last 30

years, I have been astonished at the prevalence of syphilis which has been made out by most speakers here to-day. The only one who gave any comfort to my soul was Dr. Rennie, of Sydney, and I think that this may be explained to a large extent from the general practitioner's point of view. We have heard what our worthy President has said, and perhaps he is in the very best position to judge; but, supposing Professor Allen had made his post-mortems amongst the patients of general practitioners, and not as a specialist in the hospital, what would the result have been? Would not the numbers of syphilitics be less than he found in hospital? Take the question of the specialist. It is a well-known fact that syphilis is one of the greatest causes of our troubles. Specialists get a special class of case and tell us that syphilis is fearfully prevalent. As I have already said, Dr. Rennie, as a practising physician, tells us diametrically the opposite, that it is not so prevalent. I have been medical officer of health during the last ten years for Hawthorn, and all its returns are open to-day for inspection, and I say, without the slightest fear of contradiction, that there has not been one death return from syphilis during those ten years. (A delegate.—“There may have been deaths.”) There may have been deaths; but they have not been recorded, and there should be some means whereby such cases should be recorded. I am not adducing this argument to prove that syphilis is not so prevalent as alleged, but I say, as a general practitioner, and I think other general practitioners who have given their experience will agree, that the evidence should not be of a one-sided nature from specialists and leaders of the profession. Personally, I do not think syphilis is so prevalent as has been made out. That does not alter the question as to what should be done. I agree with Dr. Mason in all his remarks. It is so serious that we cannot use too many preventive means of ridding the community of the great incubus of this disease.

DR. J. W. BARRETT (Melbourne) stated that the criticism of the discussion resolved itself largely into an estimate of the value of evidence. He had begun practice in an entirely sceptical attitude of mind with regard to the extensive diffusion of syphilis, and on clinical evidence, supplemented by post-mortem examination, had gradually come to take up an even stronger position than that indicated by the President. He believed that Gowers had referred to the faith of the eye-man in mercury. It was a faith that was justified by experience, and induced by the possibilities of observing changes and the effect of treatment on them. He did not wish to worry the meeting with a statement in detail of the data which had led him to adopt the position now assumed. It was, however, abundantly clear that a large number of the subjects of syphilis disclosed no clinical evidence of any description of the grave condition from which they were suffering. Such people, however, died from very small injuries or lesions, and the condition was discovered after death. If it be asked, why is it assumed that the lesions described are syphilitic, the answer is clear: people known to have had syphilis or syphilitic children show such lesions after death. It therefore devolved on the physicians to show that some other cause could produce identical lesions, in cases in which from social reasons the history of causation could not be definitely proved. The onus of proof lay with the objector. There were further a number of people suffering from syphilis who showed no clinical evidence except in their eyes, but the examination of the corneæ of those who had suffered from interstitial keratitis in childhood would show the vascular remains in the corneæ. The examination of others showed isolated patches of choroiditis.

as the results of attacks of kerato-iritis. Again, it became a matter of evidence. If in practice one had seen a syphilitic father married, if one had traced the medical history of the children as they grew up, if one had seen attacks of interstitial keratitis and seen patches of choroiditis produced, why should he assume that some other toxin produced identical results in those cases in which the chain of causation could not be proved, but which were controllable by mercury. Apart from accidents and from alcoholism, there was no particular reason why persons should die when young, or comparatively young, except in odd cases, and it was probable that tubercle and alcoholism were in many cases grafted on to the feeble constitution of the subjects of inherited syphilis.

In accordance with the wish of the Executive Committee, he moved the following resolution:—

That Syphilis is responsible for an enormous amount of damage to mankind, and that preventive or remedial measures directed against it are worthy of the utmost consideration.

COLONEL THOMSON (Brisbane).—I prepared a return for the Military Section here, and I entered in it the number of syphilitic cases which had appeared during the last twelve years, in the 1,200 men I have had under my own observation. We had them for two years and three years, some of them buying their discharges and so on. In the 1,200 men, there were twenty-seven cases, so that 2.2 per cent. of the men had either acquired syphilis or were treated for syphilis, and they were fine sturdy fellows, military men, well-fed, and not very hard worked. They were inspected every fortnight. Their shirts were taken off, their trousers rolled up above their knees, and any suggestion or suspicion of any secondary rash or eruption was carefully noted. (Hear, hear!)

DR. DOYLE (Newcastle).—We have listened here to-day to the speeches of a large number of specialists, and I arise to speak as a general practitioner in one of the largest seaport towns in Australia. My conclusions of a practice, extending now over something like fifteen years to twenty years, lead me to think that the statistics brought before us to-day by Professor Allen are not quite in accord with my practice. What I think has been neglected by the specialists who spoke on the question, is the fact that Australians in the coastal districts are becoming immune to syphilis. We know that in Portugal people have been syphilized for so many hundreds of years that now syphilis is a rare disease in Portugal. I remember Mr. Jonathan Hutchinson, not very long ago, referring to the fact that in time to come, just as the people of civilized communities have become immune to such diseases as measles, which work havoc in uncivilized communities such as Fiji, so in time to come people will become so immune to syphilis that it will be of small importance. A little while ago, I was looking at the statistics of syphilis with reference to the segregation of women in the Indian Army, and those figures are altogether higher than those quoted by Dr. Thomson. In the Indian Army, Lord Kitchener reported to the Council of India, and the paper was afterwards published by the House of Commons, that after the Contagious Diseases Act or the inspection of the women in the cantonments of the British Army in India—I am speaking now of the white Army in India—after those restrictions were done away with, and the examinations were not any longer insisted on, the number of cases admitted for venereal disease rose to 1,500 per 1,000. (Laughter.) I am quite right. I am quoting statistics from the report in the House of Commons,

and those statistics show that in the Indian Army every man in the Army was treated once and a half for every year venereally. So that the return shows that there were 1,500 cases of venereal disease for every 1,000 soldiers. Now that is a very serious condition of affairs. It led to this: a large number of those young men were enlisted in Scotland and Ireland, and they went back to their homes in the North of Scotland among the Crofters and the back country of Donegal, and they carried syphilis with them which they had contracted in India, and Irish and Scottish practitioners vouch that they had now seen syphilis in districts where they had not seen it before. I quote this to show how difficult it is to deal with the question of syphilis unless you are prepared to deal with it radically. Let us get all cant out of our minds. You are never likely to get any Bill through the Houses of Parliament of this country which does not deal with both sexes equally and well. (Hear, hear.)

DR. ATKINSON WOOD (Melbourne).—With regard to the statistics of the death rate, I think I can quote you something that has not been published so far and is absolutely definite. I looked through all the death certificate books for the last ten years at the Children's Hospital. The deaths signed up as Congenital Syphilis amounted to one in seven, in all those that occurred in the last ten years. That does not include deaths signed up as premature debility and so on, many of which are due to general syphilitic disease. There are many women who give family histories such as this—"First child, born dead; second, ditto; third, ditto; fourth, treated with mercury during pregnancy, bore healthy child." Perhaps we will get a family history of a poor woman who has had eight or nine dead children born to her. Cannot we do something for this woman? As a matter of fact, the word "syphilis" is banned in the Children's Hospital, and in most hospitals. We are not honest with our patients. We do not speak of "syphilis." The public do not know of the ravages of the disease, and even many of the medical men cannot believe it. I think that, at the Children's Hospital, we see more of its baneful effects than in most other hospitals. It is there, and an unmerited disease. It is held by the public and by the women that men deserve all they get when they get syphilis; and, as a matter of fact, if it stopped there, we might not have much to say. It does not, however, stop there. It goes to the families and the homes, and not only do the wives get all these still-born children, but idiots and congenital deformities of all kinds are born to them, and the mothers do not know why. If the doctor were to tell them, and point out the facts—he need not use the word "syphilis"—but if, at all events, he told them the next time they got pregnant that they should go to a doctor and be treated during pregnancy, not only would many lives be saved, but many healthy children, who otherwise would have been born idiots and deformed, would be saved too. I have drawn up a card in which I have put down several of these practical suggestions, which I propose should be given to mothers who have born syphilitic children, so that in future their offspring could be saved. It simply explains in a few words what is the condition. What has been objected to in this card are the words, "Your child is in my opinion suffering from syphilis." That might be deleted; but it would not alter the rest of the card, and it would warn the mother what to do if she got pregnant again. It has also been stated that you will never get the women to help us. I think that when the women appreciate that, in the Children's Hospital, at all events, syphilis is an unmerited disease, they will be the ones to help us, and it is from the Children's Hospital we can appeal best to them, not only to

the women, but the clergy and public in general, and it is for this purpose that I have made these remarks. We ought to be honest. Until we are honest, and call it "syphilis," with a large "S" if you like, we cannot expect much advantage from our deliberations. Let us educate the young by all means in the way suggested by the Children's Section, and if we could get it made a notifiable disease, as Dr. Mason has suggested, I think we would be within measurable distance of preventing it as we are preventing consumption at this day. [*Hands President card.*]

INSTRUCTIONS SUGGESTED BY DR. W. ATKINSON WOOD.

1. Your child is in my opinion suffering from syphilis.
2. This is a curable disease, but only by continuous treatment for 12 months, and intermittent treatment for 12 months longer, even when the child is apparently healthy.
3. You can obtain medicine for one month at the Hospital, at the end of which time you must return for another month's supply (or you can get tickets for medicine from a local chemist).
4. If this treatment is not carried out, your child may be backward, or may become blind, or deaf, or suffer from nervous, bone, skin, or other diseases in after-life.
5. The disease can be spread by contact, by kissing, by towels, spoons, or cups; therefore, the patient should not be kissed by others (except by the mother, who cannot take the disease), and should only use its own towels, cups, spoons, &c.
6. The child may readily give the disease to any nurse who suckles it.
7. If you become pregnant again, immediately go to a doctor and show him this card, and he will give you medicine which you must take throughout your pregnancy, you will then probably have a healthy baby.
8. If your child at any time becomes ill, always show the doctor this card.

THE PRESIDENT (PROFESSOR ALLEN) said:—I think it would be a great mistake if we were to concur with Dr. Bennie that syphilitics should never marry. (Hear, hear!) In that I agree with Fournier and Balzer. With proper time, delay and treatment, there is no reason why a man who has had syphilis should not be a good husband and a good father. (Hear, hear!) I think, secondly, that Dr. Webster's point is an exceedingly good one as to the relationship between alcohol and syphilis. The experience of the Indian Army lately has shown that with growing temperance in the Army there has been an enormous diminution of the prevalence of venereal disease. In the third place, I think we must not allow Dr. Mason to lead us into any approval of breach of medical etiquette in matters of disclosure. The statement that there is no breach of confidence if the law requires a breach will not commend itself to any assembly of medical men. (Cheers.) As Dr. Lockhart Gibson says, if we were to agree to anything of that kind, we should deter patients from coming to us, and do an infinity of harm. (Hear, hear!) I think these three things should be borne in mind. Syphilitics with proper delay and proper treatment may marry. Alcohol is the great friend of syphilis. In the matter of confidence, every medical man must be a judge of what he ought to reveal, and what he ought not to reveal. A medical man must not allow serious evil to be done through exaggerated regard for confidence; but medical men must never break confidence unless there is the gravest possible reason for so doing. (Cheers.)

The motion was then put and carried, with some dissent.

THE PRESIDENT.—With regard to the suggestions from the various Sections, I personally am rather unwilling that we should go too quickly. I think this meeting will have a remarkable educative influence both in and beyond the profession. (Hear, hear!) I think the wisest course would be to refer these suggestions to the Executive Committee of the next Session, and to ask it to appoint a special committee, and to take all other steps that are necessary. Very little delay would arise in so acting. Will that meet with your good will? (Cheers.)

DR. E. S. JACKSON (Brisbane).—Would not that put the matter off for three years?

THE PRESIDENT.—No. I suppose the new Executive will be formed immediately, and that, as one of its first duties, it will take these suggestions into consideration and appoint a special committee, allowing that committee to take such steps as it thinks fit, reporting to the Executive from time to time, and finally reporting to the next Session of Congress. (Hear, hear!) Does that meet with your approval?

DR. JACKSON.—Yes.

The President's suggestion was agreed to.

THE PRESIDENT then declared the Congress adjourned till Saturday, 24th October, at 10.15 a.m.

FINAL GENERAL MEETING.

SATURDAY, 24TH OCTOBER, 10.15 A.M.

(MASONIC HALL.)

The Final General Meeting was held in the Masonic Hall, Collins-street, on Saturday, 24th October, at 10.15 a.m. There was a very large attendance of members, the President (Professor Allen) occupying the chair.

CORRESPONDENCE.

(1) A letter was read from Miss Clarke, thanking the Congress for its sympathy with her mother, Janet Lady Clarke.

(2) A letter was read from the Consul for Greece, on behalf of the Consul-General, transmitting a circular stating, with the concurrence of the Kingdom of Greece, the Minister of Home Affairs, the Sheriffs of the State, and the Administrative and Municipal Authorities, the Pan-Hellenic Medical Congress had decided to convoke in May, 1909, a Greek Congress against Tuberculosis, and inviting the attendance of those interested.

(3) A letter was received from the Secretary of the Closer Settlement Office, forwarding by direction of the Hon. the Minister of Lands, a number of copies of the handbook on "Victoria and its Metropolis" for the use of members.

(4) From Lieut.Colonel Fetherston, stating that the Minister of Defence had placed the Engineers' s.s. *Vulcan* at the disposal of the Acting Principal Medical Officer, for a short trip on the Bay on Sunday afternoon.

REPORTS FROM SECTIONS.

(1) *Tropical Australia.*

DR. R. ARTHUR (Sydney) reported that the Section of Public Health and State Medicine had resolved to recommend—

That the Congress appoint a deputation to wait on the Prime Minister, and request him to appoint a Royal Commission of experts to inquire into the possibility of the colonization of Tropical Australia by the Anglo-Saxon and other white races, such deputation to consist of Dr. J. W. Barrett (Victoria), Dr. Ramsay Smith (South Australia), Dr. Elkington (Tasmania), Dr. Ahearne (Queensland), Dr. Pope Seed (Western Australia), and the mover, Dr. R. Arthur (New South Wales).

DR. ARTHUR moved that this recommendation be adopted, and stated that the deputation would be received the same morning. The motion was put and carried.

(2) *Medical Statistics.*

The Section of Public Health and State Medicine reported that it had considered the report of special committee concerning medical statistics, and had resolved to recommend—

- (1) The adoption of the Report.
- (2) That a form of death certificate which would facilitate uniform specific certification of the causes of death is most desirable, and any such certificate should be forwarded direct to the local Registrar of Deaths, who should issue a burial certificate.
- (3) That a copy of the Report be forwarded to the statist for their consideration.

The recommendations of the Section were adopted.

(3) *Medical Inspection of School Children.*

The Section of Public Health and State Medicine reported that it had resolved—

That the Congress be informed that the Section is of opinion that there is evidence to show the prevalence of physical defects among school children, and it therefore recommended that the responsible authorities in New Zealand, and in those States which have not instituted a system of medical inspection, should be urged to adopt some uniform system of physical examination of all school children, carried out under the direction of, and where necessary, by medical experts.

The Section for Diseases of the Eye, Ear and Throat reported—

That the Eye and Ear Section desires to recommend to the Congress the extreme importance of the medical inspection and examination of school children, and especially urges the systematic examination of their vision and hearing.

The recommendations of both these Sections were adopted.

(4) *Inter-State Sanitary Conferences.*

The Section for Public Health and State Medicine submitted the following recommendation:—

As recommended by previous Congresses, there is a pressing necessity for regular annual conferences between the professional heads of the State Departments of Public Health in Australasia, in order to secure, in the public interest, better co-ordination of hygienic effort, with better co-ordination in its application."

The recommendation of the Section was adopted.

(5) *Radiotherapy.*

The Section for Diseases of the Skin, Radiotherapy, &c., recommended—

- (a) That the various Health Authorities and Hospital Committees be recommended to take the necessary steps to insure that adequate protection is provided for X-ray workers at the Hospitals throughout the Commonwealth.
- (b) That this Congress strongly impresses on the profession the advisability of consulting only qualified medical men for its X-ray work.

The recommendations of the Section were adopted.

(6) *A Medical Member in Naval and Military Boards.*

The Section for Naval and Military Medicine and Surgery reported—

That in the opinion of this Section it is in the highest degree desirable for all Naval and Military Boards or Councils of Defence to have an expert medical member.

The report of the Section was adopted.

(7) *Hospital Abuse.*

DR. W. MOORE (Victoria), convener of the special committee concerning the Relations of the Medical Profession to Hospitals, proposed—

- (a) That the resolutions in reference to the Abuse of Hospitals be forwarded to all the recognised Medical Societies in all the States and in New Zealand; and that the Societies be asked to take whatever action they may deem advisable.
- (b) That a committee be formed to act with the next Executive Committee in dealing with questions of Hospital Management, and to report through that Executive Committee to the next meeting of Congress; the committee to be: Drs. Worrall and Blackburn (New South Wales), Lockhart-Gibson and McLean (Queensland); Hayward and Cavenagh-Mainwaring (South Australia); Butler and Pardey (Tasmania), Lewers and Moore (Victoria); Seed, Holland, and Saw (Western Australia); Roberton and Ewart (New Zealand).

On the motion of DR. LOCKHART GIBSON (Queensland), the name of Dr. J. W. Barrett (Victoria), was added to the committee.

The motions proposed by Dr. Moore, as thus amended, were adopted.

THE NINTH SESSION OF CONGRESS.

DR. ABBOTT, President of the New South Wales Branch of the British Medical Association, said that he had the honour of moving the next resolution—

That the next Session of the Australasian Medical Congress be held in Sydney, in or about the year 1911.

On behalf of the profession in New South Wales, he could assure the Congress that, if the resolution was adopted, New South Wales would give a hearty welcome to all the visitors from other parts of Australasia. He felt very much the kindly manner in which they had all been entertained during their stay in Melbourne, and assured the Congress that visitors went away with most pleasant memories. (Cheers.)

THE PRESIDENT put the motion, which was carried with enthusiasm.

ELECTION OF PRESIDENT.

PROFESSOR ANDERSON STUART (Sydney) said he had much pleasure in moving that Dr. R. Antill Pockley, Lecturer on Ophthalmic Medicine and Surgery in the University of Sydney, be elected President of the next Session. He spoke highly of Dr. Pockley's professional attainments, and said that there had been between Dr. Pockley and himself a very long association of the most pleasant character. (Cheers.)

THE PRESIDENT put the motion, which was carried enthusiastically.

DR. POCKLEY, on ascending the platform, was received with loud cheering. He said that he thanked the Congress most sincerely for the very highest honour that could be conferred upon any man—the confidence of his colleagues. It was particularly gratifying that he should have been proposed by Professor Anderson Stuart, with whom he had been intimately associated for nearly thirty years, since the time when the professor was one of his teachers at the Edinburgh University. He would not like members to go away with the impression that there were not men more worthy and capable for the position; he knew, however, that he would have the willing and enthusiastic help of the most able men that could be got together in Sydney. He felt it not only a duty, but a privilege to do something for the profession which had done so much for him, and it was for this reason that he accepted the position. He would do his little best, and his wife would do her not unimportant part in welcoming them at the Congress in Sydney, though they could not expect to emulate the magnificent reception the members of the Congress had met with in Melbourne. (Loud cheers.)

VOTES OF THANKS.

COLONEL THOMSON (Brisbane) moved—

That the Congress tender to His Excellency the Governor of Victoria its grateful thanks for opening the Session, and expresses its appreciation of the interest that His Excellency and Lady Gibson-Carmichael have taken in its proceedings.

The motion was carried unanimously with acclamation.

DR. LOCKHART GIBSON (Brisbane) moved—

That the thanks of Congress be presented to the Premier and Government of Victoria for undertaking to print the transactions of Congress; to the Deputy Postmaster-General for

providing a post and telegraph and telephone service for Congress; to the Railway Commissioners of Victoria, the other Australian States and New Zealand, and to the Shipping Companies for concessions to members travelling; to the Secretary of the Public Works Department, and the Inspector-General, Mr. Davidson, for providing a barge for the Congress House-boat at Henley; and to the Minister of Defence for placing the Engineers' S.S. *Vulcan* at the disposal of members on October 25th. (Cheers.)

The motion was put and carried unanimously.

DR. JEFFERIS TURNER (Brisbane) moved—

That the Congress express its keen appreciation of the hospitality shown to its officers and members and their wives by His Excellency the Governor and Lady Gibson-Carmichael, the Lieutenant-Governor and Lady Madden, the President of the Legislative Council and Lady Wrixon, the President of the Victorian Branch of the British Medical Association and Mrs. G. A. Syme, Mr. and Mrs. J. F. W. Payne, Mr. and Mrs. Robert Harper, Mrs. John Turnbull, Mr. and Mrs. J. A. Levey, the Victorian members of the Australian Army Medical Corps, the General Secretary of the Congress and Mrs. Maudsley, together with Dr. Atkinson Wood and the Committee of Victorian members who jointly provided the Congress House-boat at Henley, the Victorian Medical Women's Association, the members of the Alexandra Ladies' Club, the Victoria Racing Club, the Victoria Amateur Turf Club, the Masonic Club, the University Club, the Golf Clubs, Dr. and Mrs. G. R. Adam, Dr. and Mrs. J. P. Ryan, and all the ladies and gentlemen within and without the profession who so kindly entertained the members and ladies accompanying them.

The resolution, which was punctuated with cheers, was carried unanimously and with great applause.

DR. TRACY INGLIS (Auckland, New Zealand), moved—

That the Congress express its thanks to the Chapters of St. Paul's Cathedral and of St. Patrick's Cathedral, and to the Scots Church, Collins-street, and to the several preachers, for the special Congress services on Sunday, October 18th, and to the Council of Churches for its cordial greeting and good wishes.

The resolution was carried unanimously.

Professor E. C. STIRLING (Adelaide) moved—

That the Congress express its great indebtedness to the Chancellor, Vice-Chancellor, and Council of the University of Melbourne for the grant of the use of the University buildings; to the Professors and Lecturers who placed their rooms at the disposal of the Executive; and to the Registrar, Mr. W. E. Cornwall, M.A., and his staff, for their kind assistance. That the Congress also warmly thank the Committee of the Medical Society of Victoria for permitting the Committees of Congress to meet in the Hall of the Society; and the Committee of

Management and the Secretary (Mr. T. R. Andrews, B.A., LL.B.), of the Melbourne Hospital, for permitting the Board Room to be used as a Congress office ; and thank the authorities of all the Hospitals and other Institutions that invited inspection or facilitated work. That a special vote of thanks be accorded to all who have contributed to the formation of the Congress Museum, to the firms who sent exhibits of great value, and to the Museum Committee and its Convener, Dr. Murray Morton. (Loud cheering.)

THE PRESIDENT said that, before putting the motion, he wished to express his obligations to the Director of the University Conservatorium of Music and Mrs. Peterson ; to Colonel Evans, who had kindly permitted the band of the Sixth Australian Infantry Regiment to play at the University ; to the Electrical Department of the City Council, and particularly to Mr. H. R. Harper and Mr. Alabaster, for their great assistance ; to Major Wardill, representing the Melbourne Cricket Club ; to Mr. H. Skinner, who had carried out so excellently all the catering for the week, and by his foresight had relieved the Committee of all anxieties in that department. The President further announced that Mr. Boyce, of the Railway Department, was in the room on behalf of the Victorian Railway Commissioners, in order that the greatest comfort might be provided for members returning to their homes. (Cheers.)

The motion was then put, and carried with enthusiasm.

DR. CAHILL (New Zealand) moved—

That the Congress offers its warmest thanks to the Press for the full reports of its meetings, and for the excellent support given to the Session. (Loud cheers.)

THE PRESIDENT, supporting the motion, expressed on behalf of the Executive Committee the utmost gratitude for the support given by the Press. In the troublesome and difficult task of reporting the meetings, in the midst of busy sessions of Commonwealth and State Parliaments, the Press had treated the work of the Congress with great generosity and great skill.

The motion was then put, and carried with acclamation.

THE HON. DR. BUTLER (Hobart) moved—

That the Congress expresses its appreciation of the excellent preparations made by the various committees, General, Executive, Sectional, Museum, and Special, and particularly thank the Honorary Treasurer (Mr. G. A. Syme), the General Secretary (Dr. H. C. Maudsley), and the Associate Secretaries (Drs. Balfour, Stephens, and Vance).

The motion was carried with loud and repeated applause.

THE PRESIDENT said it gave him very great pleasure to convey the vote of thanks to the different committees, and he gladly took the opportunity of thanking the Treasurer, the General Secretary, and the Associate Secretaries for the accurate and thorough manner in which all their work had been carried out, and of expressing his personal gratitude to all those who had co-operated in promoting the success of the Congress.

COLONEL THOMSON (Brisbane) then rose to propose a very hearty vote of thanks to the President. He said:—There was once a very distinguished countryman of mine—there are many distinguished countrymen of mine here present, but the particular one I refer to was infinitely

more distinguished than you all—he wrote books—he wrote Guy Manner-
ing—he created Dominie Sampson. The Dominie, when astonished at
anything, was wont to throw his arms aloft, swing them like the sails
of a wind-mill and shout “Prodigious,” till the roof rang with his
raptures. When we remember the labours of our distinguished President—
even acknowledging the loyal support and assistance he received from his
staff of officers—we wonder. For months—years—he has been planning
and creating and collecting the necessary machinery for this great effort—he
has been assembling the various parts—he has been fitting them—he
has been lubricating them, and he got everything to work sweetly and
smoothly—with the gratifying result that this great Congress—the greatest
of them all—has been a marvellous success. Surely then, in our wonder
and amazement, we can throw up our arms, like the worthy Dominie, and
shout “Prodigious.” And the Lady President, Mrs Allen, must not be
forgotten; to her we owe more than we can ever know; she, with her
many charms, ably assisted her husband and worked enthusiastically to
make this Session the great social triumph which it is. I ask you to join
with me in tendering a very hearty vote of thanks to Professor and Mrs.
Allen. Ladies and Gentlemen:—Upstanding—the President and Mrs.
Allen with three cheers.

A scene of great enthusiasm followed, the members of the Congress
rising *en masse*, and greeting the resolution with long continued ringing
cheers.

THE PRESIDENT, speaking with much emotion, thanked Colonel
Thomson for his kind words and all the members for the wonderful
manner in which they had affirmed this vote of thanks to Mrs Allen and
himself. The labour and anxiety had been great, but the harmony and
success of the Congress was a complete reward. As his final duty, he
then declared the Eighth Session of the Australasian Medical Congress
closed.

TROPICAL AUSTRALIA.

*Deputation from Congress to the Prime Minister, Saturday, 24th
October, 1908.*

A deputation from the Australasian Medical Congress, consisting of
Drs. R. Arthur (N.S.W.), J. W. Barrett (V.), Ramsay-Smith (S.A.),
Elkington (T.), Ahearne (Q.), Pope-Seed (W.A.), and C. W. Reid
(N.S.W.), waited on the Minister for Home Affairs, in the unavoidable
absence of the Prime Minister, on Saturday, 24th October, 1908, and
presented the following resolution on behalf of the Congress:—

That the Prime Minister be requested to appoint a Royal Commis-
sion of Experts to inquire into the possibility of the permanent
colonization of Tropical Australia by the Anglo-Saxon and
other white races.

DR. ARTHUR (Sydney) thought it could be said, without fear of contra-
diction, that the most urgent and momentous problem before Australia
to-day was the filling up of the unpeopled north. The Commonwealth
proposed to take over a large area of tropical Australia, but it would be
a blunder of the first magnitude to dump large numbers of people there

without first knowing, with a fair degree of accuracy, whether the country and climate were suitable for white people to settle there. Plenty of opinion could be obtained for and against the idea. During the last two or three years it had been found possible to carry on work in the cane fields by white labour. It had formerly been declared dogmatically that this work could only be done by coloured labour. As a result of the experiment in the cane fields, we were encouraged to hope that white men might be able to engage in other branches of industry in the tropics. An inquiry into the matter would be not only of Australian, but Imperial, importance. If it were found that the Anglo-Saxon race did not take kindly to tropical conditions, then the question to be considered would be whether some other white race, accustomed to a tropical climate, might not be successful colonists. It might be necessary for the Commission to travel beyond Australia to inquire whether races that had settled in the tropics had maintained their physical and mental standard. The expense of the Commission to the Government would be amply recouped by the lessening of the possibilities of colonization that might result in disaster.

DR. J. W. BARRETT (Victoria) said they had come as a deputation from a Congress interested in scientific matters, and in no sense had they to do with politics. It was in the interests of science and their own country that they asked for an attempt to be made to secure a scientific solution of a problem by no means settled. It had been said that the Anglo-Saxon race could not live in the tropics. That had not been proved. It might be proved or it might not. Those with experience of the interior of Queensland realized that the hygienic conditions under which the people lived, and the unintelligent construction of the houses, made it difficult to test the problem by means of the experience of the white population already in the tropics. A few years ago it was considered that the yellow fever was a disease that could not be controlled. Within a few years, however, the Panama Canal zone had been cleared of the scourge, through the energetic efforts of American medical scientists and sanitarians. If the conditions of life were regulated in accordance with proper conditions of hygiene it would be possible to test the possibilities of Northern Australia. The medical profession had to thank the Commonwealth Government for the support given to the Tropical Diseases Institute at Townsville, and they now asked the Government to appoint a purely scientific and non-political commission, one of its number to be the head of the Townsville institution. Such a commission might be expensive, because it would be of little use unless it comprised scientific men of eminence.

DR. W. RAMSAY SMITH (Adelaide) said the subject was no mere speculative consideration, but a practical question of serious import. Medical men who had had much experience in such matters had formulated certain conclusions from their own observations. They said, for example, that, excluding localities in the tropics characterized by excessive heat, high relative humidity, or unhealthful soil conditions, acclimatization or physiological adaptation of the white man to tropical environment was possible; that if acclimatization were possible, then colonization was possible; and that the failures of the white man to live in the tropics and maintain health, excluding the localities referred to, appeared to have been due principally to the non-observance of the rules of personal, domestic, and public hygiene. It was becoming recognised that white visitors or settlers in tropical climates must change some of their temperate climatic

habits, just as explorers in the Arctic and Antarctic must, if they were to maintain health. Further, it was one of the greatest discoveries of modern times that tropical countries had special diseases which were acquired in special ways, and that special means had to be employed for their prevention. The recognition of these facts had enabled France and America to accomplish in their tropical possessions what a few years ago would have been characterized as wild dreams or absolute impossibilities. The fact that such an industry as rice-growing had been found to be possible under dry methods of cultivation introduced new factors into the problem of Australian tropical colonizing. The solution of the problem would involve the collection of evidence from a variety of sources.

DR. ELKINGTON, Chief Officer of Health (Tasmania), said he was not there as the representative of any State, or any political connexion, but as a delegate from the great Congress that had just completed its labours. With regard to the persistence of the white races in the tropics, he had come to the conclusion that the question of climate, *per se*, or mere temperature, had nothing whatever to do with it; that the persistence of infant life was only a matter of parental care, and that an observance of the laws of hygiene had an overwhelming influence on the welfare of the people.

DR. J. AHEARNE (Queensland) said an estimation as to whether it was possible to establish a white race in the tropics might be arrived at if they imagined all the countries between two tropical lines on the Mercator projection. Such countries would be found to be peopled by coloured races.

DR. POPE-SEED (Perth) said that unless they could succeed in colonizing by white races the north of Western Australia, which was rich in fertile tracts, in time to come people of another colour would come in and occupy the territory. Certain parts of Western Australia which, in the early days of the gold-fields, had been thought to be unfit to sustain European life had proved to be no worse than many flourishing inland cities of Australia. When Coolgardie started as a gold-field it had been said that no white woman could live there. In due course, epidemic diseases that had been prevalent had disappeared as the result of improved methods of life and attention to hygiene. He hoped that thousands of miles of territory inhabited by white people would be added to the west and other parts of Australia as the result of an investigation such as they sought.

DR. C. W. REID (Sydney) thought it would be feasible to colonize the Northern Territory with white people if proper attention were paid to personal hygiene, proper housing, and feeding. If people dressed themselves properly in the tropics there was no reason why they should not live there decently and comfortably.

THE MINISTER FOR HOME AFFAIRS, in reply, said the Prime Minister was detained at an important Cabinet meeting, and had asked him to hear the views of the deputation. Although Dr. Barrett had said that their representations were not made in a political sense, he presumed his meaning was that it was not a matter of party politics. (Hear, hear.) It was a national question. He was not prepared to say that the Government would agree to the appointment of a Royal Commission, but the request would receive full and, as far as possible, favorable consideration. He agreed with Dr. Barrett that they should not forget the work done in connexion with the Panama Canal. It had been an object-lesson to the world that medical science properly applied to existing conditions

could convert and transform them so as to make it possible for white people to continue living in places where, without the application of that medical science, and without that industry and attention to hygiene, it would not be possible for more than probably 10 per cent. to survive. If that were done in Panama, in India, and on the west coast of Africa, surely the conditions in Australia could not be so shockingly bad that similar work on the part of our medical people here could not make it possible to colonize our tropics with a white race. He agreed that this matter would have an Imperial importance. The Commonwealth Government had done something towards the investigation of tropical diseases. It voted, and intended to continue voting, an annual sum for the Tropical Diseases Institute of Townsville (hear, hear), and it also contributed towards a similar institution in Great Britain. That showed that the Government was alive to its responsibilities in investigating this important matter. The representations of the deputation might result in something satisfactory being done. At any rate, early and careful consideration would be given to their request.

The deputation thanked the Minister and withdrew.

CONJOINT MEETINGS OF SECTIONS.

1. Papers and Discussion on Tubercle, in the Sections of Medicine and of Pathology and Bacteriology.
2. Papers and Discussion on Anæsthetics, in the Sections of Medicine and of Surgery.
3. Papers and Discussion on Peritonitis, in the Sections of Surgery and of Obstetrics and Gynæcology.

TUBERCLE.

PAPERS AND DISCUSSION IN THE SECTIONS OF MEDICINE AND OF
PATHOLOGY AND BACTERIOLOGY.

THURSDAY, 22ND OCTOBER, 1908, 11 A.M.

DR. G. E. RENNIE, President of the Section of Medicine, in the chair.

TUBERCULO OPSONINS.

JULIAN SMITH, M.D. MELB.

The evolution of our present-day concepts of bacteriotherapy began with the recognition of the fact that infective diseases are due to the invasion of the body by bacteria. The second essential idea was that of the specificity of infective disease. That is to say, that whatever predisponents are operative, no infective disease arises, unless the definite

bacterium causing that disease becomes established somewhere in the body. The correlation of these ideas with the all-time fact that an attack of an infective disease often confers immunity, more or less permanent, gave birth to the prophylactic bacteriotherapy of to-day.

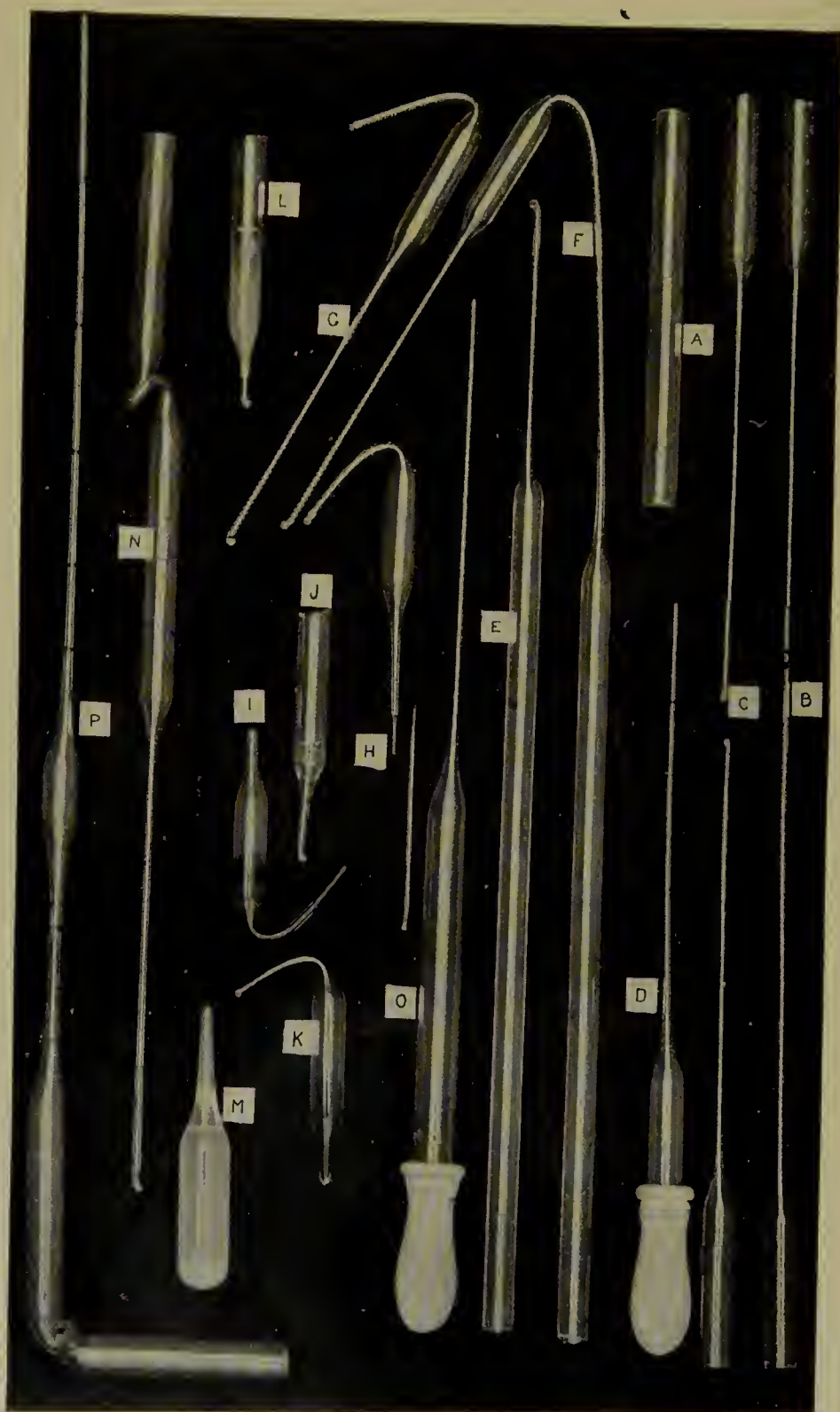
Jenner applied these principles, unconsciously at any rate, to the prophylaxis of small-pox. Pasteur's monumental work includes much of the same sort of thing. Koch, however, made a distinct advance when he observed that small doses of dead tubercle bacilli arrested tubercular disease of some rabbits. He was the first to attempt the cure of an infective disease by the use of vaccine. His early attempts on human beings were not happy, but later modifications have largely remedied early defects. It remained for Wright, however, to rationally complete the evolution of bacteriotherapy.

Beginning with the use of a prophylactic typhoid vaccine, he developed methods of measuring the bactericidal power of serum. Finding that serum or plasma had no bactericidal effect on certain other pathogenic bacteria, he was led to make inquiry into the nature of immunity in these diseases. Being aware of Metschnikoff's views as to the cellular theory of immunity and of Leishman's early primitive methods of making comparative phagocytic measurements, he, with an infinity of detail, so modified the early methods of measurement as to show that the plasma takes a part of vital importance in the scheme of immunity. As each new fact bearing on immunity was brought to light, it tended to sway opinion accordingly. Thus, with the advent of Behring's phenomenally potent antitoxin, the efforts of a large number of medical scientists were turned to attempts at the production of other equally potent antisera, both anti-toxic and anti-bacterial. The results have been very poor in comparison with those achieved in the case of diphtheria. Had not original inquiry been thus diverted by the anti-diphtheritic work, the use of vaccines would probably have come about much sooner. As a matter of fact, immunity results from many contributing factors, which are not brought to bear in the same relative degree on each and every disease. In one, the anti-toxic defence is the main one called forth, and if in default death ensues however lusty the anti-bacterial defence, and *vice versa*. Moreover, it seems clear that immunity to any one disease does not necessarily arise by identical processes in dissimilar species.

Those diseases in which the defence is largely anti-bacterial have long been the subject of controversy. One side maintained that the humoral theory was the correct one, and, indeed, the increased bactericidal power, *e.g.*, of typhoid patients' serum, seemed conclusive. But the phagocytosis observed both in *vivo* and in *vitro* lent ample support to the cellular theory of immunity.

It remained for Wright to unravel the skein. He ingeniously separated the body cells from the body fluids, and presented the cells to a suspension of bacteria at blood heat for varying periods. The resulting phagocytosis was practically negligible. He then returned the body fluid to the bacteriocellular mixture and again incubated it, and found that a very effective phagocytosis resulted.

In order to be sure upon which of the two cells or germs the fluids thus acted, he exposed germs to body fluid for some time, separated the germs from the fluids, and again presented them to the cells in an inert saline medium. The phagocytosis was equally efficient, and Wright came to the conclusion that anti-bacterial immunity was in part due to a property of body fluids, which rendered bacteria approachable and



absorbable by body cells. This property he designated opsonic for obvious etymological reasons. Wright soon found a remarkable numerical deformity in the degree of phagocytosis in repeated experiments on the same serum, and at once saw the value of this in comparing the antibacterial powers of different sera. The ratio of this power of a serum to that of a presumed normal sample he termed the opsonic index of the serum.

In the earliest experiments, the blood fluid was isolated by shedding blood directly into a solution of sodium citrate in normal saline and centrifugalizing; a citrated plasma resulted. Later inquiries showed, however, that serum separated by clotting fulfilled requirements. So that the simpler process of shedding blood into a glass capsule and using the serum is now universally adopted. This "capsule" is made from ordinary glass tubing. A length of tubing is taken, fused at A in the blow-pipe flame, and drawn out as at B, being fused through at C, and sealed there. D is discarded. The tubing is again fused at E and drawn out once more. Just as the glass is solidifying at F, it is bent into a curve as shown at G. The capsule is liberated by fusing through at H. The process is repeated again in the glass, so that several capsules are made from the one piece of tubing without waste. The capsules thus made are sterile, and are laid aside in numbers so as to be ready when required. When put into use, the end *r* is broken off, and the other end fused thoroughly on a small pilot light, then taken from the flame and quickly drawn out as shown. By breaking off this filament, a sharp sterile needle point remains, admirably suited for puncturing the skin. I always choose the back of the thumb close to the nail and make one or two quick stabs which are practically painless. The thumb is then constricted from the base downwards by several turns of bandage or tape, the sharp end of the capsule is broken off, and the syphon end presented to the drop of blood as shown. Should the blood not flow freely, the bandage should be loosened, the arm made dependent for a minute, or swung round vigorously, and then pressure reapplied. The thumb should be warm. I think it well to at least half fill the capsule with blood. It should be so held that no blood runs to the straight end. The sealing of the capsule is an important little matter, and is the source of many failures with beginners. The capsule is held with the curved blood end between the thumb and fingers, which extend just beyond the blood. The hollow air-filled portion is then quickly warmed in the pilot light and the straight end of the capsule is sealed at once. As soon as this sealing is done, the warmed glass must be rapidly cooled. The position of the fingers insures that the blood is not heated, a vitally important point. As the heated air within the sealed portion cools, it contracts and the blood follows it, so that the unsealed end is left empty to beyond the shoulder. If the blood does not retreat thus far, the sealed end must be further cooled under the tap. The capsule is now held by the curved end and shaken down like a clinical thermometer, so that all the blood goes to the straight end. The curved end is then sealed carefully and the capsule laid aside until required. The separation of the serum is hastened when necessary by incubation at blood heat, and sometimes centrifugalization will be called for. The capsule is finally broken through at the shoulder with a file used as a lever, just before the serum is required for experiment.

Corpuscles.—It is a tenet of Wright's, in the main true, that it is a matter of indifference from what source the corpuscles are obtained, to supply the second element in the opsonic test. Theoretically, at any

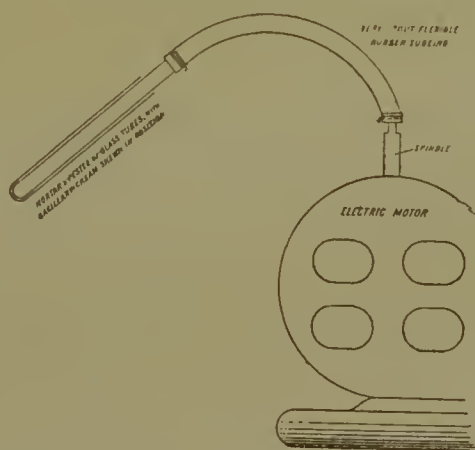
rate, it should be so, since it is the powers of sera that are being compared and the corpuscles take the part only of indicators, it being assumed that there is practically no spontaneous phagocytosis in the absence of serum. This point will be referred to later. But apart from this, I find that corpuscles vary in ability to preserve their outline whilst being stained, and again some bloods contain many large mono-nuclear and transitional forms, which in some films leave one in doubt as to whether they should be included in the count. Besides this, the red cells of some bloods have the property of being agglutinated by certain sera. If this is at all marked, it produces an error so great as to render the test, with that blood used as the source of corpuscles, valueless. This will also be reverted to later on.

A suitable source of blood having by trial been arrived at, from ten to fifteen drops are shed, directly into a solution of 1.5 per cent. sodium citrate in normal saline. The thumb is pricked and constricted as before described, and held with the distal joint flexed and pointing downwards, so that the blood drips from its tip directly into the citrated saline. The receptacle is inverted every now and then with one's thumb over the top, so as to thoroughly and quickly mix the blood with citrate, and prevent clotting. Various receptacles have been suggested, but I prefer the ordinary conical glass tube of full size supplied with clinical centrifuges. It is filled two-thirds full with citrate, thus amply diluting the plasma, and its wide mouth rapidly receives the blood without its touching the sides. The corpuscles are now separated from the citrated plasma by centrifugalization. This must be neither too violent nor too prolonged, as the corpuscles are then apt to form clumps not easily dissociated. No rule can be given, but practice will soon settle the point. Our own electric centrifuge gives a good result in five minutes. Inspection now shows a red deposit of blood cells about half-an-inch deep, sharply defined from a supernatant fluid slightly hazy from blood platelets. The plasma is pipetted off from the cellular deposit, care being taken to avoid stirring the latter. This is best done by holding the tube on the slant and carefully directing the point of the pipette towards the upper segment of the tube very close to the corpuscles. So soon as this is done, the tube is part filled again with normal saline and the corpuscles thoroughly mixed with it. This so greatly dilutes any remaining plasma as to render it negligible. A second centrifugalization again throws down the cellular elements as a red mass and the supernatant saline is removed as before. The upper layers of the remaining red mass are rich in leucocytes. These are now skimmed off with the pipette and thoroughly mixed up in a watch glass, and are then drawn up into a pipette and sealed off as shown. This last step insures that each volume of corpuscles used in the test contains a sufficiency of corpuscles, whereas if volumes of corpuscles are drawn up direct from the top layers of the original deposit, some are apt to contain very few leucocytes and counting becomes difficult.

The Centrifuge.—A good centrifuge is essential for the performance of opsonic work. It is quite possible to use a hand machine, but this entails considerable labour and is a waste of time. Water-driven centrifuges are now on the market, and are so far improved as to run reliably. The electrically-driven apparatus with rheostat attachment for speed variation is also very satisfactory, but fully equipped is apt to be costly. The writer has adapted the arm and buckets of a hand machine to the spindle of an electric fan motor mounted vertically. This works directly from the main, and with the three speeds already attached proves

very effective. It is important to remember that good results are obtained only by accurately balancing the load in the buckets; it takes but a moment to adjust this on suitable scales always at hand, and is quite worth it.

The Emulsion.—The preparation of the third element in the test, a suitable suspension of tubercle bacilli, is the most exacting part of the long and tedious business of opsonin estimation. The bacilli must be neither too numerous nor too scanty, and ideally there must be no clumps. It is not always convenient to grow tubercle bacilli, and a far readier source is the preparation of dead organisms placed in the market by Allen and Hanburys. It is in the form of a cream, bacilli in 1.5 saline, sterilized by heat. Thus obtained, the bacilli are clumped and need dissociation. Earlier methods consisted in grinding the cream in a mortar or between plates of ground glass, a tedious process. Latterly, I have found two methods equally efficient in giving good emulsions. One consists in improvising a grinder from two pieces of glass tubing, closed roundly at one end and fitting snugly the one within the other. The inner one is connected to the spindle of the electric motor by several inches of stout rubber tubing, and is thus driven at a rapid rate as a revolving pestle. A small portion of the bacillary cream is placed within the larger tube and rapidly triturated for ten minutes or so. The outer tube is held in the left hand and the revolving grinder in the right, which also steadies the rubber connexion. After the grinding, the outer tube is part filled with saline and the contents mixed. A thorough centrifugalization is now carried out, and the supernatant fluid will, or should be, clump free. It will probably be too thick, but it may be stored in this form and a small portion of it diluted down as required. The most suitable decanting and storing apparatus is a piece of glass tubing blown into the form of a pipette as shown. A sufficiency is drawn up and the end then sealed.



If such a capsule be laid aside and examined next day, it will be seen that all bacilli have settled, but a brisk shaking will re-suspend them and a portion may be withdrawn by a sterile pipette after removing the tip of the capsule; this may again be sealed or a sterile rubber teat may be used as a cover. All of the foregoing steps may be performed in a sterile manner, so that it will be unnecessary to sterilize the emulsion by boiling, which is very apt to re-clump the bacilli. Should, however, it be suspected that contamination has occurred, a short heating is not very harmful, beyond that the final centrifugalization may need to be more thorough. Certain organisms will multiply in the emulsion, and would interfere with the test. The suitable strength of the emulsion can with

practice be fixed upon by the appearance of it. It is advisable to examine it under constant conditions of light, and it is then a reliable means. The eyes should be shaded from the light which traverses the emulsion, this being held against a sombre background. The appearance then seen is as of a faint haze. Beginners will find it safer to make a preliminary opsonization smear and staining to settle the point. One or two trials will show what amount of dilution and subsequent centrifugalization are necessary in order to get a suitable emulsion. These conditions adhered to each time a test is made greatly expedite this part of the test, and are good until that particular supply of emulsion is exhausted. It will be seen that by thus preparing a quantity of emulsion at leisure, there is a great saving of time. The withdrawal of the quantity required for each day's use is made in an aseptic manner by means of an ordinary pipette and subsequent dilution, if required, made in a watch-glass, the mixture being sealed off as shown. It is always advisable to have the mixture so strong as to finally require a couple of minutes' centrifugalization, since clumps are less likely to be retained. Care must be taken that the little tube is placed vertically within the centrifuge bucket. If dropped in in the ordinary way, it rests on the slant and there is a deposit of bacilli along one side of the tube. This might be the source of inaccuracy if this deposit were disturbed by an opsonic pipette during the subsequent tests. I find that if the rubber teat is allowed to remain on the glass tube, it remains vertical within the bucket and this fallacy is avoided. Nevertheless, as a final precaution, the emulsion is sucked up into another clean pipette and sealed off, care being taken not to disturb any deposit. This last tube may now be twirled as often as desired without fear of stirring up clumps, and there is a certainty that all portions of the emulsions are uniform. If it has been standing for more than a few minutes, I think the twirling ought to be done, because the upper layers otherwise soon become attenuated and there would be uncertainty as to each opsonic pipette getting the same strength of emulsion.

Another method which I find gives a very good stock emulsion is in effect a shaking one. The crude bacillary cream is sealed in a glass tube with some glass beads, and the tube is agitated for some hours. I have adapted a small electric motor for this purpose. A small wheel attached to the armature spindle has a rod fixed to it eccentrically; the other end of the rod plays in a loose bearing. The sealed tube of beads and emulsion is held fast to the rod by rubber bands and makes excursions of a couple of inches, two or three hundred to the minute. After the shaking, many bacilli are still, of course, clumped. The tube is centrifuged for a few minutes to deposit the coarsest clumps. The supernatant emulsion is still very thick. Some of this is withdrawn in a sterile manner and mixed with sterile saline and again thoroughly centrifuged. This supernatant fluid is finally drawn up into the storing pipette before described, care being taken to avoid the sides in order to escape stray clumps. I have described the preparation of the bacillary suspension at such length, because I think it is the most fruitful source of error in the test. It is futile to place any sort of reliance on reading obtained by the use of an imperfect emulsion.

The preparation of various pieces of glass apparatus used in the test is important enough to merit some description. One's need for these is so constant that it is very inconvenient to be dependent on outside supply. If possible, then, a gas supply and foot-worked blow-pipe should be procured, and efficiency at elementary glass-blowing should be attained. Practice will bring this about more readily than any written description.

The Pipette.—This consists of a piece of glass tubing of outside diameter of about two-thirds of that of an ordinary lead pencil, drawn out at one end into a capillary tube about two-thirds the size of the lead. To prepare a supply of these, a piece of glass tubing is cut into 3-inch lengths, and each piece makes two pipettes. It is fused thoroughly at its middle by slowly revolving it in a steady blow-pipe flame. It is held by its ends between the thumb and fingers of each hand, and when red-hot is taken from the flame and slowly drawn out to the required length. It will be found that very little force is required at the beginning of this manœuvre, but as the glass cools more and more will be required, until just at the finish, quite a strong pull is exerted. This increase must be gradual and regular, and it is only in this way that a uniform bore is obtained. An endeavour should be made to get all the pipettes of the same bore. Practice will render this possible. The longer the glass is held in the flame, the smaller will be the resultant bore. So soon as the drawn-out tube is set, it is fused through again at the middle, and each end sealed as shown. The stock of pipettes are laid aside in this form until required for use. The tip is then squarely removed with a small file or glass cutting knife, and a reference mark placed on the glass about an inch from the end. A blue grease pencil serves well; it is best to hold the capillary tube in the left hand and to revolve it firmly beneath the pencil held in the right hand. A transverse clean-cut mark is essential. Brunswick black, smeared on with a fine glass thread makes a splendid mark. A rubber teat is now fitted to the base of the pipette and it is ready for use. In manipulating the pipette, the neck of the teat and the glass within it are held fixedly between the thumb and first and second fingers, and the suction and propulsion of contents brought about by slight variations of pressure of the ball of the thumb.

In proceeding to the opsonic test, all the elements must now be conveniently assembled. Various devices for holding the glass containers have been suggested. I find a flat block of wood, a few inches long and 3 inches wide and 1 inch thick, very suitable. Small holes are bored in it obliquely and at regular intervals on the top, and these receive the butts of the capsules and present their openings upwards and to the right, so as to be easily entered by the pipette held in the right hand. The various capsules containing the sera to be tested are now opened and placed in order along the block, with the corpuscles and emulsion at the near end. The pipettes are prepared and placed near at hand, also a sufficiency of clean microscopic slides. I put these direct from the packet into 3 per cent. acid alcohol. They are removed just before use and wiped free of alcohol with a clean, non-fluffy rag, care being taken to leave them thoroughly polished throughout. It is well to have a little more than twice as many slides as pipettes. Reserve smears are often a great comfort.

In proceeding to the test, the pipette is held with great delicacy and steadiness as described, and equal volumes of corpuscles, emulsion, and serum drawn up in the order mentioned. The volumes are separated by air spaces. They are then carefully discharged on to clean watch-glass, thoroughly stirred together with the end of the pipette, still held as described between the thumb and fingers. The mixture is drawn in and ejected from the pipette a few times until it is seen that mixing is complete. The mixture is then drawn in for the last time free of bubbles and held stationary in the middle of the capillary tube, whilst the end is carefully sealed in a small flame. The teat may now be released from the fingers and removed from the pipette. The pipette is then placed in a special incubator, as shown, kept at blood heat. The incubator may shortly be described as a small brass box, through the face of which are placed

horizontal water-tight brass tubes, which receive the pipettes. They are surrounded by water placed in the box and kept at constant temperature by a thermostat dipping into the water through the top of the box. The heat source is a small gas burner placed beneath.

The period of incubation may vary at the wish of the worker. Fifteen minutes is a favorable time, but longer periods up to half-an-hour insure a greater number of corpuscles becoming phagocytic, and with thin bacillary emulsion gives a greater count.

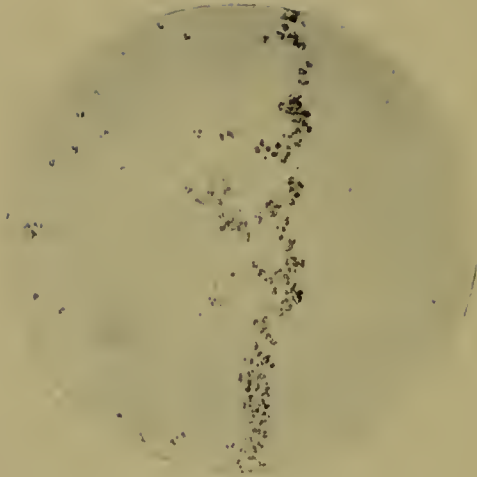
In performing a series of tests at the one sitting, the pipettes are placed in the incubator in turn when completed, and are withdrawn in turn at the exact times at which their respective incubations are mature. Many methods have been adopted to insure accuracy—electric clock contact, a clock with two minute hands at right angles, and so on. I employ a stop-watch, and start it at zero when the first pipette is inserted, at the same time recording on paper opposite its name the number 15 or 20, as the case may be. Then if the second pipette is put in when the stop-watch records 1 minute 45 seconds, the number opposite the name of second pipette is 16.45 or 21.45, as the case may be, and so on. A convenient interval between each pipette is two minutes. Each pipette is withdrawn as the watch records the time proper to it, the sealed end removed with a file and the contents carefully blown on a clean watch-glass, thoroughly mixed by a stirring motion of the pipette, the large end being still between the lips. This is further assisted by sucking up the mixture into and ejecting it a few times from the pipette, when this mixing is seen to be complete, I think this should be equally well done with each pipette in order that a thoroughly typical drop in each case is used in making the smear.

In making the smear, a drop three or four millimetres in diameter is placed near the left end of the clean glass slide, held in the left hand. The edge of another clean slide (ground edge) held in the right hand is placed on the drop, which runs at once into a long thin line between the two slides, making an angle with one another. The upper slide is then drawn slowly to the right and the lower to the left. The smear will begin to form, and when it is seen by careful watching that all the drop is just used up, the slides are carefully parted. If the manœuvre is successful, the edge of the smear is thin and straight. Both of these qualities are essential for accurate and rapid counting. If the terminal edge is too thick, the phagocytes appear small and globular and overwhelmed with red cells; if it is too thin, the phagocytes are burst. The happy mean requires considerable practice, but when successful is very satisfactory.

Another useful method in making the smear is the employment of a special spreader. This spreader is made from an ordinary glass slide. Two small file marks are made exactly opposite one another, one on each long side of the slide, and the slide is then broken across. After several attempts, one slide will break, so that the broken edge is just the least bit concave. This edge is then used in smearing, and it is thoroughly cleaned after each spread. The smear made with this is tongue-shaped, and the leucocytes are sought for along the free curved border. Each slide is identified by writing the name across the surface of the smear, just as it is drying, with a blunt-ended wooden match.

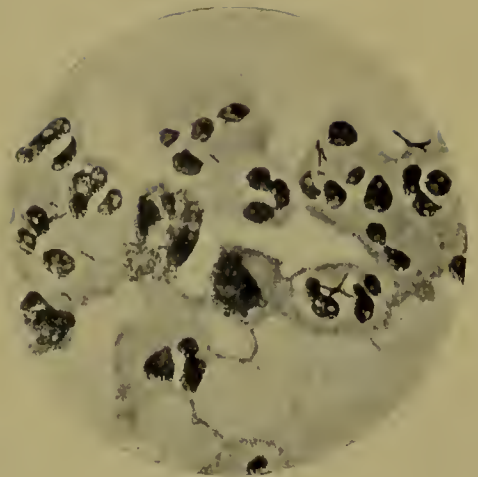
In fixing and staining, the same attempt at instituting uniform conditions throughout should be made. Thus all the slides are fixed at the same time in a receptacle holding them in slots. Saturated solution of perchloride of mercury is the best fixator, and five or six minutes' immersion will suffice. This is poured back and used again indefinitely. The

I.



Low power view of free edge of film showing fringe of leucocytes, discrete, unruptured, yet well segregated, for easy counting.

II.



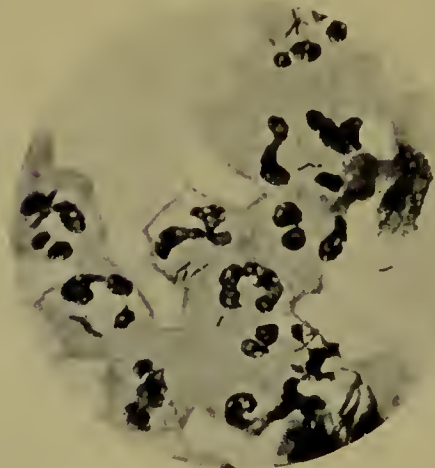
High power view of part of edge shown in Fig. I.

III.



Another high power view of Fig. I.

IV.



High power view of satisfactory film.

V.



High power view of satisfactory film.

VI.



Showing large branched form of bacillus ingested

slides remain in the same receptacle until completely stained. Then they are washed thoroughly in tap water, which is drained off well. Ordinary carbol fuchsin warmed to 50.C is then poured in and allowed to remain for about a minute. It is then returned to the bottle and may be used again several times. The excess of stain is removed from the slides by washing, and a solution of 1 per cent. sulphuric acid in water is now poured in and remains for twenty to thirty seconds; this may also be used again when required many times. The acid is thoroughly soaked out in tap water and the smears should now appear colourless.

In the final step of counter-staining, an alkaline solution of methylene blue is used for a few seconds. I find that $\frac{1}{8}$ gm. blue, $\frac{1}{4}$ sodium carbonate in 100 c.c. distilled water is sufficiently intense, the outline of the cells being very distinct. The staining power of the solution must be gauged by experiment, as it varies with age. It may be used over again a few times, but deteriorates sooner than the fuchsin. Too deep counter-staining obscures the bacilli and may be toned down with a dash of spirit. Before drying off the slides, they are better examined under the low power to decide upon correct counter-staining. They are then thoroughly drained and rapidly dried by standing on end or may be blotted dry.

The opsonic test, an exacting one at all stages, calls for particular care at the counting. It is here that workers have confessed to great difficulty in deciding what values to place upon appearances.

One difficulty arises from meeting with bacilli of such varying lengths that one organism has eight or ten times that of the other. Then, again, clumps appear which have been obviously ingested at one gulp, and it may be that only a portion of the clump was opsonized and was instrumental in the inclusion of the whole. It is also objected that the outline of the cells is so indistinct, even invisible, that no certainty exists as to the inclusion or not of bacilli close to polymorphonuclear cells.

In attempting to answer these objections, it must be remembered in the first place that the index is a ratio between two numbers, each amounting to a hundred or two at least, and that if certain precautions are adopted, the possible error due to the above objections will not be great, and in the actual ratio (index) still less. First, then, what is sauce for the goose is not amiss for the gander. So long as the same rules for counting are applied to each and every slide, the indices which are *comparisons* of slides should tally. For instance, assuming that the bacillary emulsion has been made very uniform by the precautions before mentioned, the same proportion of long and short elements occurs throughout, and whether the long ones are called two, or, on the other hand, the short fragments ignored, or even the same value given to all lengths, the ultimate index should be the same. Clumps, if frequent enough, ought perhaps to come under the same argument. But if rare, and one chances to come in a thin emulsion, it had better be ignored entirely. As a matter of fact, counting slides with many clumps require a much larger count, and is only making the best of a bad job. In properly prepared emulsions the difficulty does not arise.

With regard to the inclusion of bacilli in a cell or not, there can be no difficulty in a properly prepared and stained film. But even when the cell outlines are not perfectly shown, the close proximity of bacilli to polymorphonuclei and their complete absence from the surrounding field is so suggestive as to be a certainty in the mind of any one who has examined thousands of more or less successful smears, and should the wrong decision be made, as indeed it must be at times, ordinary expectation

makes it likely to occur as often in one slide as another, and the result remains unaffected. The accompanying illustrations show what should be aimed at in the preparation of films.

To give uniform conditions, the same illumination must be used for all the counts, and they should be made by one observer. The count is much quicker if some one else scores. With good smears, the contents of a hundred cells may be estimated in from seven to ten minutes. The scorer places the numbers in columns and rows of ten each, so that a glance shows the number of cells recorded. When the observer is alone, he mentally records the contents of cells until five or ten are considered, and the result is then put to paper. He then sees at a glance when he has reached the proposed number of cells, and the mental additions save time. It has been pointed out by some observers that the count varies according to the part of the slide selected. Personally, I look for a part of the free edge, with the low power somewhere near its centre, where the cell outline is good and the cells discrete, and I adhere to this position in all cases. The minimum number of cells to be counted compatible with accuracy seems to vary amongst workers. As a fact, the number is influenced a great deal by the degree of perfection of the technique. The experienced worker finds on some days some element in the test somewhat below par, recognises it and makes allowance accordingly. Thus his opinion may be that everything is so perfect on one day that fifty cells will give an accurate result; on another, he may not be satisfied with less than a hundred cells. Since the index is a *ratio*, and the *pool* count enters into it in every case, it is well to endeavour to get the pool count particularly accurate, and thus I usually count twice as many cells on the pool count as on the others. It has been said in many quarters that even with all care, very divergent results arise during control experiments. I cannot say that my experience agrees with this. Given satisfactory technique, I have no hesitation in saying that a series of independent estimations of the index of a given serum will agree within very narrow limits, usually the variations being within .05. Its accuracy as a laboratory test in expert hands is, to my mind, unquestionable, but whether the index is a true measure of the complete body resistance is an entirely different matter, which probably needs, as I hope to show, further inquiry.

It will make things clearer if a few examples taken at random are given. One day's counts will be given in full. The identity of the slides was unknown until the counting was complete. The indices of A, B, C, were estimated practically as control tests, for they were healthy individuals. D was a case of tubercular disease, undergoing tubercular treatment. E was a case suspected to be tubercular.

Pool.	1st Slide.	
7 2 3 . .	2 4 3 . 5	... 26
2 1 . 7 3 2 1 .	1 3	... 20
4 2 . 3 3 1 3 .		... 16
2 1 2 . 2 8 . 2 .		... 17
2 3 . 2 1 2 2 1 .	5	... 18
3 1 5 2 . 2 4 4 2 .		... 23
1 3 1 2 3 1 3 1 1 2		... 18
3 3 1 1 2 1 2 . 2 3		... 18
3 . . 1 3 1 1 . 2 .		... 11
. 2 2 3 1 1 2 1 4 .		... 16

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Pool.	2nd Slide.	
. 3 . . .	4 1 . 6 1	... 15
3 . . 1 2 1 2 4 .	3	... 16
3 5 3 1 . .	2 3 2 .	... 19
1 3 5 . 3 3 .	6 1 .	... 22
4 3 3 5 .	1 5 2 .	... 23
3 1 2 . 1 1 2 .	3 2	... 15
1 1 3 . .	4 2 3 .	... 16
2 5 2 2 4 . .	4 4 3	... 26
3 3 1 2 2 1 . . .		... 12
4 . 3 2 . .	3 3 2 2	... 19

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Such close agreement cannot be regarded as anything more than coincidence. The real point is that counts, which total about 200, rarely differ by so much as twenty, the average being from ten to fifteen, and when the effort is made to diminish this error in *pool*, one factor in the index by counting more cells, errors in the other count produce negligible error in the final result.

The counts of the other slides on the day ran:—

A.—17, 30, 21, 22, 20, 12, 14, 13, 20, 13 ... 182
 B.—21, 14, 23, 14, 29, 14, 13, 17, 26, 12 ... 183
 C.—25, 22, 13, 20, 17, 15, 14, 13, 15, 17 ... 171.

A and B were active, were in full work; C was a perfectly vigorous man with enlarged prostate, with no suspicion of tubercle about him.

D.—18, 10, 24, 13, 20, 20, 22, 25, 32, 21 ... 205

E gave 130 organisms in 100 cells, *i.e.*, index 71.

The following is another control movement made by two observers adhering closely to the same technique, but separate pipettes, smears, staining, and counting with unidentified slides, were employed.

By A.	Phagocytes.	Organisms.	Index.
Pool ...	100	420	
Patient ...	100	342	.81
By B.			
Pool ...	100	424	
Patient ...	100	342	.80

Again on 4.11.07, A made pool 100 297.

B made it the same..

On 7.11.07. A made pool 100 215
 and 100 211

B made pool 100 208

14.11.07. The various results of pool were—

100 248, 251, 249.

30.11.07. 100 251, 249

2.12.07. 100 142, 159

5.12.07. 100 207, 227

9.12.07. 100 198, 193

23.12.07. 100 250, 248.

Examples could be multiplied indefinitely. Taking a case where the divergence is more marked, and on this day, the emulsion was somewhat uneven, we get—

19.10.07. 100 (A) 162 bacilli
 100 (B) 148 bacilli
 100 (B) 159 bacilli
 100 (B) 174 bacilli

4/644

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On ordinary chances thus, 160 bacilli per 100 cells is pretty near the absolute truth. The second count is probably ten too little, and the fourth fifteen or so too much. The same deviations from the absolute are open

to chance in the case of the patient's count, but if, as has been pointed out, the pool has been made to approximate the truth closely, errors in the other count do not materially affect the index.

Thus, suppose pool 160 absolute

and patients slide 130 absolute. and index .81

Then it may happen that the first count of patient 120

or it may happen that the first count of patient 140

and that the index be recorded as ... $\frac{120}{160}$.75

or as ... $\frac{140}{160}$.87

And this degree of accuracy is quite enough in ordinary therapeutic work, where one wishes to know whether the index is just high or low, and be thus guided in the treatment. Where on the other hand, one wishes to approach the absolute closely as in fine points of diagnosis, I think it better to correct both the pool count and the patient's. And after all, it takes very little more time, granted that the technique is perfect at all points.

Enough has been said to show the degree of accuracy to be expected from the test. There is one point of importance yet, and that is that occasionally a slide will be met with giving uniform counts for each ten cells, and then a complete blank is met with for as much as twenty cells in one part of the slide. This may be due to chance and unequal mixing, but more often is due to patchy staining, as careful restaining will demonstrate.

The accuracy of the laboratory phenomena having thus been settled, it will be of interest to record here various experiments which throw some light upon the nature of the opsonic action. By varying in turn each of the three elements of the test, some facts of interest come to light.

Thus in a certain pool smear, 100 cells contained 225 bacilli.

By making as the only change the corpuscular suspension half strength with normal saline, 100 249 bacilli.

By making as the only change the bacillary emulsion half strength with 1 per cent. saline, 100 cells 105 bacilli.

From several experiments it appears that alterations in the number of leucocytes make very little difference, except that the count averages somewhat higher in the higher dilutions. It is as if there were plenty of bacilli in all cases for the phagocytes present.

It also appears that the count is roughly proportionate to the strength of emulsion. Whatever the nature of the interaction between cells and organisms with serum as intermediary, there is a directly proportionate increasing difficulty at ingestion, as the bacilli grow scantier in suspension.

When the third element, the serum, is diluted, the other two remaining constant, the result is surprising, as these figures show:—

Thus—

Pool ...	... 100,	162, and 100	167 bacilli	
Tubercular patient	100,	207, and 100,	215 <i>i.e.</i> index	1.27
Pool serum ...	... 4,	100,	108, <i>i.e.</i> index	.65
Pool serum ...	... 16,	100,	55, <i>i.e.</i> index	.33
Patient's serum	... 4,	100,	154, <i>i.e.</i> index	.93
Patient's serum	... 16,	100,	81, <i>i.e.</i> index	.49

Again on another day, regarding a serum undiluted as index 1 we have—

Pool— 2 index	.92
— 4 index	.71
— 8 index	.57
—16 index	.49

And many other experiments confirm the fact that in dilutions of four and higher, the index is proportional to the square root of the dilution or the plotted curve of dilutions and indices as ordinates and abscissæ, is a Parabola.

Some observers have occasionally found that low dilutions ($\frac{1}{2}$) have given to an index higher than the undiluted serum, though I have not found this. But at any rate, there is not a proportionate diminution of the index in low dilutions, and some other factor in the interaction of the elements must have a preponderating influence.

Passing now to the effect of heat on the opsonic power of a serum, we may say that a serum loses most of its efficacy during the first couple of minutes of heating to 60 C. Wright states in one of his earlier works that the serum of an infected patient, or one having recently had tuberculin, does not lose by any means all its power, even when heated for ten minutes to 60 C., and he quotes several cases retaining as much as .8 of index, and went so far as to employ this as a diagnostic test. If any serum retained an index of .2 or over, he regarded it as tubercular. I cannot say that my experiments confirm this as a reliable test. Though it has proved correct sometimes, in others the index of heated serum in undoubted tubercular cases has been under .2 considerably. Certainly per contra, all non-tubercular sera have proved to be very low indeed, practically nil.

In this connection Deane has pointed out in other experiments an apparent contradiction of Wright's interpretation of the heated serum test, which is that opsonin is thermolabile. Deane says that this is true only within the narrow limits of Wright's test, as made with pipettes. Deane, instead of directly testing the heated serum, first allowed it to act in excess on the organisms for some time in the incubator. He then separated the organisms and made a *thick* emulsion of them in saline, and then performed the opsonin test by mixing this thick emulsion with corpuscles and incubating for fifteen minutes. He found a considerable count in some cases, the average in some being sixty germs per cell. Apart from the uncertainty of such abnormal non-vital condition as so thick an emulsion, and also the difficulty of being accurate with such gigantic cell contents, Deane's method is certainly reliable for showing the presence of small amounts of opsonin. To my mind, it does not seem to show more than that; in fact, working with very thick emulsions, phagocytosis arises by Wright's direct method too with heated serum, and of course with the unheated serum, the cells can do no more than fill themselves, so that comparisons are impossible. As bearing on this question, I have performed the experiment of mixing bacilli (ordinary strength of emulsion) with the serum, and incubating the mixture for an hour. This mixture was then withdrawn thoroughly, stirred up and two volumes added to one of corpuscles in pipette and the ordinary opsonic test completed. At first glance, one would expect that readings would be higher than with the same test made direct, without pre-incubation. On the contrary, it appears to be always somewhat less, and this is true of

normal serum, immune serum, heated and unheated, diluted and undiluted. I thought perhaps that those of less potency would be able to catch up to the stronger sera when allowed to act beforehand for some time on the bacilli. It would seem, therefore, that with the same serum and bacilli, the degree of phagocytosis depends upon the time that the three elements are together. This observation seems to throw a glimmer of light upon the nature of the action. Some contend that the serum acts on the bacilli alone, but although no action occurs in the absence of serum, it would more adequately express the condition to say that a plus capillarity is set up between cells and organisms by the serum. This fact, that predigestion of serum with bacilli at blood heat produces a lessened phagocytic count, when corpuscles are finally added and opsonised, may be explained by supposing that the serum has a bacteriolytic effect, and therefore would be also bactericidal. If this is so, it shows that immunity to tubercle is in part made up of a bactericidal action.

The next point of interest to determine is the question as to whether unheated serum can reactivate heated serum. To this end, normal and immune sera were diluted up to sixteen times with saline and their indices estimated.

Secondly, instead of saline, heated serum, normal and immune, was substituted for the saline as the dilutant. Within the limits of error, the numbers showed that heated serum and saline could be interchanged in all cases. Had reactivation of opsonin occurred, the readings of the sero-serous dilutions should have all been high instead of progressively diminishing *pari passu* with the sero-saline dilutions. That is to say that the mixture of one part serum and fifteen parts heated serum, gave an index practically identical with the mixture one part serum and fifteen parts saline. These observations seem to be rather in favour of the opsonic power being a distinct property of serum.

Phagocytic Potency.—Mention has already been made of the fact that the source of the corpuscles in the opsonic test is a matter of indifference. This is because the test is a comparison between sera. In experiments made by Wright, several varieties of corpuscles appeared to possess equal potency, so that, in his opinion, the opsonic test was a true measure of blood resistance.

The following experiment illustrates the point. The index of a patient was estimated twice over. The conditions were exactly similar, with the exception of the corpuscles. These were prepared with equal care from two healthy individuals, and used in turn.

A's Corpuscles.

Pool— 100 cells, 162 germs, and 100, 167 av 165
Patient—100 cells, 207 germs, and 100, 215 av 210

B's Corpuscles.

Pool—100 = 170 and Patient—100 = 214.

So that the index resulted in 1.27 in the one case, and 1.25 in the other case. And roughly, that is the general experience about this inquiry. But if this is so, one must conclude that those people who are prone to tubercular infection have a poor serum fortifying mechanism. But is this really so? One sees cases in which the rise in index in response to inoculations is good, and yet the lesions, which may improve, do not clear up completely; nay even others appear. As bearing on this question of cell resistance, the following inquiries are of interest. A patient with pleuritic effusion due to colon septicæmia was tapped. The fluid was

separated from the cells of the exudate, and the latter washed in saline and re-suspended. They were largely polynuclear. The patient's index to tubercle was .93, and that of the exudate .89, and these values were arrived at twice over, both by using healthy blood corpuscles, and then again the pleuritic corpuscles, but the actual members of the two series had the ratio of 127 to 98. That is to say the blood corpuscles were more active than the pleuritic cells. Then again, a further experiment along the lines of this inquiry was made by comparing the corpuscles of an individual apparently robustly healthy, but with a well-marked tubercular family history. Several devices were followed. One plan adopted was to opsonise whole blood mixed with bacilli in 1 per cent. sodium citrate. An emulsion of bacilli was prepared in 1 per cent. sodium citrate in normal saline. A pipette was then taken, and one volume of this emulsion drawn up, and directly following it one volume of freshly shed blood from the finger. Rapid mixing prevented clotting, the mixture was then drawn into the pipette and sealed as usual, being incubated after the usual manner. In one control experiment, three pipettes were made for each of two bloods, one (B) being that of the individual just mentioned.

A's Blood.

- No. 1 Pipette—100 cells, 266 organisms.
- No. 2 Pipette—100 cells, 232 organisms.
- No. 3 Pipette—100 cells, 259 organisms.

B's Blood. (Tb. family history.)

- No. 1 Pipette—100 cells, 211 germs.
- No. 1 Pipette—100 cells, 211 germs also.
(second slide)
- No. 2 Pipette—100 cells, 184 germs.
- No. 3 Pipette—100 cells, 205 germs.

Identity of slides concealed till afterwards. This experiment seems to show that B's whole blood has a somewhat less resistance than A's. On another occasion, trial with the same blood showed practically an equality between them. But later still, B's blood again showed a considerable falling-off, on one occasion so much as the numbers 260 for A, 147 for B, and again compared with C's (healthy family), the same inferiority existed.

But the remarkable point of the inquiry was to follow, because on the occasion when the low reading from whole blood was obtained, the index as estimated by the ordinary opsonic test was high, 1.2, and on one occasion nearly 1.3. There is no evidence of a tubercular lesion at present in B. These indices were obtained by using corpuscles other than B's. When B's were used, the index was practically normal. B's serum did not appear to agglutinate the reds of A. One lot of readings were—

- A corpuscles plus A serum—100 cells, 167.
- A corpuscles plus B serum—100 cells, 195 (confirmed by many counts.)
- B corpuscles plus A serum—100 cells, 170.
- B corpuscles plus B serum—100 cells, 164.

On another day, controls were made between B and C, as follows:—

- B corpuscles plus C serum—100 cells, 86 bacilli.
- B corpuscles plus B serum—100 cells, 68 bacilli.
- C corpuscles plus C serum—100 cells, 97 bacilli.
- C corpuscles plus B serum—100 cells, 125 bacilli.

All these experiments suggest that B's whole blood resistance has a tendency to be below par, and that the deficiency must in great part be due to lesser cell activity. Moreover, B's corpuscles in his own fluids seem not to be so good as when suspended in a foreign serum. Want of opportunity has prevented further inquiry, but I feel that this question of cell power demands careful investigation.

It seems to me, however, that this method of measuring blood resistance direct is a very natural proceeding. So far as I have gone, control experiments with several pipettes give uniform results. It has the advantage of being a much quicker test than the ordinary serum opsonic test, for preparation of both corpuscles and serum is dispensed with. On the other hand, the patient must be quite close to the laboratory to supply freshly-shed blood, unless the old plan of citrated blood capsule be adopted. Moreover, in getting the pool count, a pipette has to be made for each blood used in the pool. This controls accuracy, but of course takes more time. I find that it will not suffice to mix each of the pool samples after incubation to supply a single smear, because the extra time which elapses before the composite smear can be made causes the count to mount up, e.g. from 252 to 295 in one case of three pipettes, and from 200 to 249 in another of three units, so that smears must be made for each element of the pool. This method of investigation may give an explanation of the tardiness of recovery of some cases under tuberculin treatment.

Agglutination.—All workers of opsonins must have observed that occasionally a serum is met with which rapidly agglutinates the red corpuscles used in the test. Personally, I soon became impressed with the fact that most of these sera gave high readings, and I imagined that the agglutination served to bring the bacilli in closer proximity to the cells. The following figures illustrate the point. On one day, it so happened that three sera agglutinated F's corpuscles; A and B moderately so, A more than B, and C markedly agglutinated them.

With F's corpuscles, the readings were:—

A—100 cells, 145 germs; B—100 cells, 117 germs; C—100 cells, 140 germs..

On further trial, it was found that these sera did not agglutinate W's corpuscles. These were the readings with the same emulsion of bacilli using W's corpuscles instead of F's:—

A—100, 71. B—100, 72. C—100, 80.

These latter made the indices slightly subnormal, so that the conclusion was that the agglutination had fallaciously raised the readings. This is exceedingly annoying at times, as it involves much extra labour in finding corpuscles that are not agglutinated. Sometimes a dilution, fourfold or so, of the offending serum, removes the property sufficiently, but even then, this involves the making of a diluted pool also.

Diagnosis.—Of the value of the opsonic test in diagnosis, there can be no question, but it is hedged round with certain limitations which must be carefully set forth.

In the first place, it is necessary to explain what is meant by the true normal index. If a number of sera from healthy individuals be opsonised side by side, and the number of bacilli ingested per 100 cells be counted, it will be found that none of the counts differ very much from the average of them all. If the average of them all, or of any one of them, be arbitrarily called unity, then the others will have indices above or below unity, but not very far from it. If a pool be made from such sera from

the same individuals, on successive days, and a standardized emulsion used day after day, the counts of this serum for 100 cells closely tally. So that it is a justifiable proceeding to use such a composite serum as a standard. Of course, it is unlikely that the standard is absolute; indeed it would be hard to devise a standard in a vital phenomenon even if *in vitro*; still various pools used in different localities cannot differ widely if care is taken to compare each contributor in turn with the pool on a few successive days.

Earlier observations led workers to the conclusion that all healthy sera had indices situated somewhere between the extremes of .80 and 1.20. But I think that further observation with improved technique must narrow the limits to .90 and 1.10; at any rate, if several readings on successive days were uniformly below or uniformly above these limits, the suspicion of abnormality of index would be strongly aroused.

Now a single reading of the opsonic index of the serum of a tuberculous patient may be below the normal limits, within them, or above them. If it is below, it is thought to be from one of several reasons. First, it may be so as the result of an auto-inoculation of tubercular toxin from an active focus. This fall, Wright called the negative phase; it may be very transient if the dose is small, or grave and prolonged. It is followed by a rise which usually exceeds the former level. In cases where the focus of disease is localized, several successive readings may show the index to be uniformly below normal. This persistent low level is supposed to precede infection and to be the cause of it, though I do not know that this contention has really been proved by observation. On the other hand, it is quite open to suppose that a small lesion may so continuously rob the blood of opsonin as to keep the index down. I remember a case of nodular lupus of the forehead in a woman of 14-stone weight, which had persistently resisted all forms of treatment, and finally gave no better encouragement under rays and tuberculin in small doses. The index would rise, but no local improvement occurred, and the index persistently returned to .7 or thereabouts. Ultimately, the lesion was excised very widely, and with great care. The operation was completely successful, and there has been no recurrence after eighteen months. After the operation, the index returned to normal, and remained there, though her health was, to all appearances, the same as before the operation. When the index is within normal limits, it may be so because the blood has just been drawn at a time when it is passing through normal on the up or down grade. Nevertheless, it has frequently been my experience to get a persistently normal index in cases of cured or arrested tuberculosis. So that the persistent low index in the tubercularly prone is not constant. It is open to suppose, however, that deleterious influences more easily bring about a fall.

Finally, a tuberculous serum may have an index above normal, and this rise is the response to an auto-inoculation from an active focus. Sometimes, however, the index will show no marked deviation from normal until the lesion is thrown into greater activity by bodily exercise, or even manipulation, such as operation or massage. With these preliminary remarks in mind, the following rules as aids to diagnosis may be set down:—

1. A single reading below .8 but near it is suspicious, two running below .8 make the presence of a lesion almost certain. One reading below .7 makes the presence of a lesion certain.
2. Several readings persistently below .90, in the absence of any other obvious depressing cause, renders the presence of a lesion somewhat probable.

3. When the index is within normal limits, endeavours should be made to produce a tissue exchange by exercise or massage, if the lesion permits. A couple of readings should be taken shortly afterwards at intervals of an hour or so. If variations beyond normal occur, the case is to be regarded as tubercular.
4. When a therapeutic-sized dose of tuberculin produces a distinct negative phase, the case is tubercular.
5. A single high reading, *i.e.*, above 1.2, renders diagnosis certain; the higher, the more convincing.
6. A wide variation on successive readings is proof positive of tubercular infection of an active type.

It will be interesting to quote some examples exemplifying these points. The accompanying figure is the opsonic chart of a patient who up to the time of the present illness had been in arduous professional duties for many years. During an epidemic, he contracted influenza, normal at its inception, but not clearing up as expected. At the end of a week, the temperature began to rise, and an eminent consultant pronounced it a case of protracted influenza. There was a vague discomfort about the right chest, but no pain; a few rales present were considered the remnant of the bronchitic trouble. Then suddenly acute right-sided pleurisy supervened, and resolved without effusion in ten days, the temperature subsiding in three days. On reviewing the whole case, the probable clinical diagnosis was tubercle. The X-ray photo. showed a marked mottling of the right apical region, probably due to fibrosis, and the respiratory excursion was here slightly diminished, though there was no evidence of active disease. The patient did exceedingly well, and with great hygienic care is better than ever. Still, with variations between 1.5 to .6 within a few days, there is no doubt that there was a tubercular flare set up by the influenza, though it does not by any means follow that the pleurisy was tubercular. A patient injured a knee by a severe blow at sport. Recovery was delayed and incomplete. A careful consideration of all points rendered a diagnosis of tubercle probable. It was decided to drill the head of the tibia for relief of the osteitic trouble. The drilling confirmed the osteitis, and the following index readings confirmed its tubercular nature:—

Two hours after drilling, .86.

Four hours after drilling, .66.

Six hours after drilling, .89.

A girl of fourteen, with scoliosis, suddenly developed pain in the side and high temperature. On two occasions, four days apart, with the temperature high, the readings were .6 and .67. A fortnight later, after tapping and recovery, the index was 1.05. The exudate was almost completely lymphocytic.

A man of twenty-five suddenly developed pain in the side. A dry pleurisy was present, and quiescent signs at the right apex. He said that a doctor told him "to sleep out" two years ago, but that he had been well until this attack. His index was 1.4, and he gave a marked tuberculo-ophthalmic reaction.

Two other sera supplied from cases of primary pleurisy gave indices of 1.45 and 1.53, and were pronounced tubercular in consequence. A young woman developed several firm-raised subcutaneous nodules. Here was a diagnosis of possible multiple sarcomata, but having in view a marked family history of tuberculosis, her serum was given to me for

examination. It proved to have the very high index of 1.7, and a certain diagnosis of tubercle was made. Subsequently one of these nodules was excised; microscopy confirmed the diagnosis. The primary focus remains undiscovered.

A vigorous young woman with a good family history had several months of harassing nursing work. Her knee became painful and swollen, but despite several weeks of complete rest, the trouble returned, in a measure, so soon as she resumed activity. When seen, the appearances were such as to suggest a tubercular arthritis of mild degree. The index, taken twice, was just .9. On a third occasion it was .88, so she was instructed to take a gentle walk of half-an-hour's duration. The serum was procured early the next day, and gave an index of 1.36. The subsequent history of this case confirmed the diagnosis of tuberculosis.

I have had two cases of chronic prostatitis following gonorrhœa which did not clear up when given every chance. Being led thereby to a suspicion of a secondary tubercular implication, and from other considerations, I made observations, a few hours apart, after a moderate prostatic massage. In both, a change in the index of .4 within a few hours confirmed the tubercular nature of the trouble. Conversely, many cases have been tested which proved not to be tubercular. One or two examples will suffice:—A man, formerly healthy, developed a high continuous temperature with some points possibly suggesting tuberculosis. Two specimens were taken, one before a rigor, and one after a rigor. The counts were:—

Pool—150 cells, 315 organisms.

Before rigor—150 cells, 312 organisms.

After rigor—150 cells, 330 organisms.

So that tubercle as a cause was thrown out. The case was later shown to be a streptococcal septicæmia.

This maintenance of a normal index during acute illnesses, not tubercular, has been a very constant finding at my hands, and is a most striking phenomenon.

BACTERIOTHERAPY OF TUBERCULOSIS.

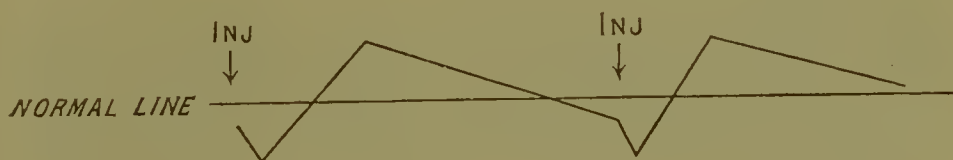
The history of this part of the subject begins with Koch's observations on this disease in rabbits. He found that if he gave a subcutaneous dose of dead bacilli, the infected rabbit as a rule died, but that if the dose was a very small one some of the rabbits survived the infection. It was found, however, that the dead bacilli remained long unabsorbed where injected, and often resulted in abscess. To overcome this, and before using the treatment on humans, Koch filtered the glycerin broth culture free from bacilli and concentrated the filtrate into what was called old tuberculin. The rise and fall of this therapeutic remedy is well known, the fall being due to two things, one intrinsic, in that it produced marked local reaction; the other extrinsic, and due, as Wright later showed, to great over-dosage.

Old tuberculin is an exotoxin. Later, Koch turned his attention to the production of an endotoxin, and his efforts resulted in Koch's new tuberculin, T. R. It was made by grinding up fine tubercle bacilli until all the organisms were disintegrated. The resultant powder was shaken up in water and allowed to stand. The supernatant fluid was then run off and came to be known as T.O. The residue was further ground up until the whole became intimately suspended or dissolved in more water with glycerin added. This was called new tuberculin of the T.R. brand.

It was put on the market by Lucius, Leister and Co. in phials containing 1 c.c. of fluid, which was said to contain 10 m.g. of tubercle powder. At any rate, Wright in his earlier work understood this to be the case, but it has lately transpired that these phials contain only 2 m.g., so that all the earlier figures on dosage must be divided by five. Of course, this makes no difference to the conclusions drawn, though it increases one's original surprise at the smallness of the effective dosage.

The difficulties in the preparation of T.R. makes it both costly and of uncertain strength, so that it has rightly been abandoned in favour of Koch's tubercle powder. This is made by giving the bacilli a complete grinding and then suspending the whole of the resulting powder in water and glycerin. One c.c. of the fluid contains 5 m.g., and it may be obtained in 1 and 5 c.c. pials. On standing, a sediment results, but it may readily be re-suspended by vigorous shaking. So far as I have observed, this "bacilli emulsion" produces no more local reaction than T.R., and gives just as satisfactory rises in the index. The dosage varies between 1-10,000 and 1-1,000 of a millegramme. In preparing the vaccine for use, several suitably-sized glass bottles (say 50 c.c.) are obtained. A quantity 50 c.c. of normal saline with .5 per cent. carbolic acid is placed in these containers and the openings sealed by fitting rubber caps to them. They are then sterilized. The first step in the dilution of the original vaccine is now taken by drawing 1 c.c. = 5 m.g. of it into a sterile syringe and then squirting it into one of the 50 c.c. bottles of carbolized saline through a puncture made by the syringe needle in the rubber cap, 1 c.c. of the fluid having been previously withdrawn. The bottle is now heated for one hour to 60 c. Each c.c. contains one-tenth m.g. of bacilli emulsion. Now, if half c.c. from this bottle be injected into a fresh bottle of carbolized saline, each c.c. of that will contain 1/1000 m.g. of bacilli emulsion, and from this various fractions of a c.c., as the case may be, are drawn as required. It is often convenient to go right on, after preparing the diluted stock quantity, and to put some of it up in sterile glass capsules, in which, so far as I am aware, it will keep indefinitely, if all the previous operations have been carried out in a sterile manner. When giving an injection, I use a Strauss syringe made by Collin, of Paris. It has a glass barrel and an asbestos plunger, and is very durable. I sterilize it by drawing up into it several times boiling water just before use. A drop of lysol is placed on the rubber cap of the bottle and the sterile needle plunged through it. A sufficiency of the contents is withdrawn and measured off on the plunger rod graduated in fractions of a c.c. The tripital or other convenient region is rubbed with 1 in 20 carbolic, or is otherwise sterilized, and the dose injected as in the giving of a hypodermic, it being desirable to well enter the fatty layer. It has already been pointed out, when discussing the value of the index, in diagnosis, that an auto-inoculation of tubercular toxin results in a fall of the tuberculo-opsonic index. The same thing is observed when a dose of the tubercular vaccine is given, and in the one case, as in the other, the gravity of the fall and its duration are dependent upon the size of the dose, but there is this important difference between them—the former may not be the origin of a single dose of toxin, but of a series. It is a variable and often progressive origin. Its variability is a disadvantage, in that it may overdo the dosage or exhibit a second dose too soon, and its progressiveness is a disadvantage, in that by generating new tubercular products and bacilli, which fix opsonins as soon as formed, the chance of an efficient opsonic rise may be continually postponed. With

the vaccine, however, there is a fixity of conditions. The whole of the immunizing response is allowed to develop without the deterrent effects just mentioned. This immunizing response Wright has described as consisting of a preliminary negative phase or fall, followed in due course by a gradual rise to a summit from which there came a gradual decline to the original level, or a little above it. As illustrating this, graceful curves were published like this—



But it should not be concluded from this that every case receiving its ideal dose goes through evolutions so regular, nor did Wright imply as much. What any one finds, who works long enough at the subject, is that there is a rough conformation to this rule. That is to say, with a suitable dose, that a fall will always be discovered from a few to twenty-four hours, and that for the next few days the generality of readings will be above normal, though, *e.g.*, the third day may register lower than the second or fourth, and that towards the end of a week or ten days the average of readings will have fallen to normal or below it, but still somewhat above the original reading. In these original experiments, Wright found that Koch's doses were much too great and also too frequent, and he showed that some of the bad effects, at any rate, were due to the negative phase lasting several days as a result of over-dose. This is, of course, assuming that the index is a complete indication of the total body resistance. Some authorities assert, however, that it is not only necessary to raise the anti-bacterial power, as evinced by opsonin, but also to raise the resistance of the body to tubercular toxin, and this, they say, requires larger and larger doses of toxin, greater at any rate than those directed by Wright. The guide of dosage is set out to be absence of marked rise of temperature, which at best seems a poor and unconfirmed measure of anti-toxin. In the face of Wright's abundantly confirmed experiments, it is unquestionably safer to adhere to his dictum of small dosage, because it will avert at any rate the undoubted calamities that are met with under the heroic dosage *régime*.

All cases of tuberculosis may be divided for convenience of consideration into three groups. In the first group, the index will be found to keep at a practically constant level. In the second, it is non-fluctuant except at intervals. In the third, it is continually fluctuating. These may be explained by saying that in the first there are no auto-inoculations, in the second that they are occasional, and that in the third, auto-inoculations are frequent. A member of the first group may be converted into one of the second or third by irritation of the focus of disease mechanically or from secondary infection or by a depression of body resistance to tubercle from intercurrent disease or by some occult cause. A member of the second or third groups may be converted into one of the first through a limitation of the focus by naturally increased resistance, by limitation of the circulatory exchange, by bodily and functional rest and by the giving of calcium or magnesium salts.

Group No. 1. Completely localized tubercle.—A typical case would be one of lupus or of quiescent lymphadenitis or of properly rested tubercular joint. The level in these cases is usually below normal, but not invariably so. In any case, a few successive readings show it to be practically constant. It is to be noted, however, that any case presenting otherwise unexplained rises of temperature should be accepted with some reserve until a greater number of readings exclude the possibility of there being auto-inoculations.

Now Wright found in such localized cases that he obtained the most satisfactory immunizing response by the small doses above mentioned. In the general run of cases, a dose of these dimensions could be found by which the undesirable negative phase lasted for a day only and that the positive phase lasted for eight or ten days, or even much longer, so that, roughly speaking, on nine days out of ten the index was raised to normal above it. Thus, as a result of his original investigations with the index guiding him, it appears now to be a safe clinical rule to give the small dose he indicates at the safe interval of ten days, without any recourse being made to index readings at all.

In the absence of such readings, as will generally be the case for one reason or another, this proceeding will probably be the best under the circumstances, but if clinical progress is distinctly unsatisfactory, I question the advisability of continuing without the index as a guide.

Granting the validity of Wright's contentions about the great advantage of maintaining the greatest summation of high index in cases of tuberculosis, every one treating such is faced with the necessity of making frequent opsonic estimations for each case. For unless he does so he is, as Wright tellingly remarks, like the mariner who attempts a difficult and unknown navigation, uncharted and without soundings. Charts he may not have except those vague ones constructed from experience in other waters, and without soundings, he may split upon a rock at any moment.

If the therapist have many cases, the whole of his time would be spent in a well-appointed laboratory. How many are there who have all of the time, skill, and application for this?

From most quarters comes the urgent demand for a compromise, for a simplification in the technique or, at any rate, for fewer estimations. The advantages of keeping a close watch on a case by means of frequent index readings are these—

- (a) A more intelligent understanding of the progress of the case is afforded. Some fact of general importance may be brought to light by sufficient study.
- (b) The most efficient dose for the case can be more easily settled.
- (c) The earliest possible time for the giving of each dose can be settled and the greatest good insured. That is to say, the interval is not necessarily always the same.
- (d) Warning of unfavorable turns in the case will be given long before clinical signs can do it.
- (e) When unfavorable clinical progress is being made, it will show whether this is due to feeble opsonic response or to some other cause.

No one will venture to question these advantages; but I doubt very much whether any system of opsonic estimations and of tuberculin inoculations, however complete, will succeed in directing a certain percentage of unfavorable cases towards recovery. Personally, many other urgent

duties have compelled me to adopt some such plan as the following:—

When a case is first seen, a few readings are taken a few days apart, before any treatment is given, so that the measure, so to speak, of the case is taken. Then a minimal dose of vaccine is given, and the effect watched by readings taken three or four days apart. By two or three such repetitions an idea is gained as to the size of dose most suitable and also as to the interval. Then only occasional readings are taken, at such time, for instance, as when an increase of dose is to be considered, or when clinical progress is slow or at a standstill, or if the patient complains of not feeling so well, even if the local improvement is good. In this way, the labour is very much diminished, though of course, points of importance may be overlooked. In addition, however, to the use of the index as a safe guide to dosage, it has a distinct value in one or two other directions. Thus, should it be found that whatever dose is employed, the opsonic rise is a feeble one, or that it is not maintained at a high level sufficiently long, experience shows that the prognosis is not favorable. Again, it is very valuable in assisting to the decision whether a case rendered functionally inactive as a means of treatment, should again resume activity. If activity no longer brings about changes in the index, already mentioned to be the result of auto-inoculation, the resumption of function is a rational proceeding and not, as was formerly the case, always a little bit empirical and fixed by time limit.

Group No. 2. Cases of intermittent auto-inoculations.—As examples of this might be quoted an advanced case of genito-urinary tubercle, early pulmonary tubercle, or a joint in use. It seems rational to suppose that attempts should be made to prevent these auto-inoculations, so that the vaccine may play uninterruptedly on the machinery of immunization. There is plenty of proof to show that rest to the part helps this. Thus, it is rational in the treatment of phthisis to have absolute repose until the opsonic level is steady, and then cautiously to allow increase of movements. If any great variation results from any increase, this last must be abandoned for a time. In this connexion, it has been suggested that phthisical patients may be given auto-inoculations intentionally by indulging in a temporarily greater degree of activity, followed by rest for some days, and until the index readings show that another self dose is required. If it can be managed, it has the advantage of giving the patient the very vaccine of his own germ, removing any doubt which may exist about the identity of different strains of organisms.

This fact of auto-inoculation having a salutary effect in eliciting high index is a very real thing. I have had occasion to report on the blood of several cases of very early lung tubercle for physician friends, and I have been very interested to observe that they have sent their patients to the country without tuberculin, trusting to the known fact of a generally high index and careful instructions given as to exercise. It is well to point out here that the conditions of pulmonary circulation render the possibility of auto-inoculations very great, and I understand also that small doses of tuberculin do produce quite evident local reactions in lung cases.

A point now comes up for consideration—Should other treatment be adopted while tuberculin inoculations are in use?

It has already been mentioned that functional activity should be arrested as much as possible, and all treatment towards that end should be followed out. Thus, the joint should be fixed, there should be rest in bed for the bad case of cystitis, and so on for each case. But should

more active surgery step in at all? I think that there can be no question that it can render invaluable service. Take, for instance, tubercular glands of large size. There should be the usual removal of all sources of irritation from the peripheral area and a regulation of the hygiene. If a few weeks of inoculation do not give distinct encouragement, it is advisable to remove the largest masses. The wounds do perfectly, and the frequency of recurrence is much lessened on account of the previously raised index. Then again, where softening of tubercular tissue has resulted, the same shortening of the treatment will result at the hands of active surgery. So far as joints are concerned, the surgeon should err on the side of conservation, turning out only those parts obviously requiring it, without proceeding to a full set operation in each case.

Also in the treatment of bone disease, resulting in deep sinuses, no cure can be expected until carious bone is removed and free drainage supplied. Another adjuvant to treatment was suggested by the fact that the fluids from tubercular products had low indices. It was as if the tissue exchange from blood to focus was not quick enough to keep up the opsonic pressure in the lesion, so that artificial means of increasing the tissue exchange were adopted. Whether suggested by this or not, it seems certain that the X-Rays produce a condition of stimulation of the tissues, whereby a greater lymph flow results and the lupus patch, *e.g.*, is flushed more strongly with the antibacterial stream.

So also when Bier applies his suction apparatus to a sinus, he increases the flow through the infected tissues of the elements of defence, and thus the sooner brings about the undoing of the invaders. It has been suggested also that where lesions are thoroughly localized, this flushing action will be safely assisted by the use of citric acid and citrates given internally. These remedies have the effect of diminishing the coagulability of the blood and thereby favoring a more ready lymph flow.

Group No. 3. Cases of persistent auto-inoculations.—By this is meant of course cases undergoing such large self doses of toxin that the index is subject to great changes at short intervals. Probably most cases are like this at some time, but they soon localize the mischief. The unfavorable cases will be found to have an index of .5 say, one day, and within a day or so, it may be 1.5, or *vice versa*. These very sudden variations, if persistent, are a grave sign, and imply serious spread of the disease. They are seen typically in advancing lung tubercle for instance, and in tubercular septicæmia. All efforts toward localization and surgical eradication failing, the question arises should these unfavorable cases be given the chance of vaccine treatment? There is a certain amount to be said in favour of it. The objection may reasonably be urged that the patient is over-dosed already, but then it must be remembered that these large doses diluted through the blood stream may not have the same stimulating effect on immunization that a dose given hypodermically would have. Wright holds that a large part at any rate of the new production of opsonin takes place locally at the site of inoculation. Remembering also that this dose is a dead and fixed quantity, it is fair to infer that this will be an unhampered factory of opsonin, quite different in kind from the active progressive lesion. These arguments are worth bearing in mind when occasion arises, as they have some evidence in their favour in connexion with the benign influence of vaccine on streptococcal septicæmia.

It is now appropriate to pass to the consideration of the various types of actual cases coming under the writer's notice during the vaccine *régime*, and to give an account of conclusions drawn therefrom; the subject may perhaps best be approached by considering the history of some cases.

The first is that of an apparently robust young man, who, five years ago, had the right testicle removed for tubercular disease. For over four years he remained in good health. Then the tissue at the side of the finger nail became inflamed. Despite the usual precautions, the trouble spread to the base of the nail and beneath it. This part slowly improved, but then the back of the finger itself began to suffer, until there was an oval ulcerated surface on the dorsum, nearly 2 inches long, painful and very suggestively tubercular in appearance. Four months had now elapsed since the beginning of the trouble. All local treatment was unavailing, and recovery seemed as far off as ever. The patient now received three injections of tuberculin, small and suitably spaced, with the surprising result that the whole finger healed promptly. The events were so timed that the very certain conclusion was that the healing was the result of the tuberculin inoculations. Now, within a month of the healing of the finger, the patient felt some discomfort in the old operation scar, a slight induration appeared, which gradually progressed, the skin over it finally becoming reddened. Under cocaine, this indurated mass, including the skin, was widely excised, the old stump of the cord being included. An incision through the mass after removal showed that it was composed of the stump of the cord chronically thickened and riddled with small pockets of tubercular pus. The wound healed by first intention. The patient is fat and well two months later. He has received only the three doses of tuberculin already mentioned. Such a history is a striking comment upon so-called cure of tubercular disease. The probable interpretation of the groin incident is that the disease was here only arrested, and that the inoculations, small as they were, produced a local reaction sufficient to upset the balance of local resistance in favour of the bacilli. The case affords, further, a good example of a sound surgical principle. Had my senior colleague been satisfied with simple incision, relying on the raised opsonic resistance to expedite cure, he would have been fortunate to obtain healing inside of months, and then with a feeling of insecurity. It is not always possible, however, from the nature of the case, to attempt total excision. Finally, this case shows well how amenable to cure by tuberculin some cases of skin tuberculosis are.

The next case, purposely chosen, is that of a man of 50, who, three years previously, had a perinephritic abscess drained. This was shown to be of tubercular origin, and the wound closed down in due course to a minute sinus, which discharged intermittently, and finally healed. Two years later the testicle became enlarged and painful, and the scrotum ultimately adherent and reddened. The tissues of the cord were infiltrated, and the seminal vesicle was easily palpable and apparently infiltrated. Bladder symptoms were absent. The obviously disorganised testis was removed with as much as possible of the cord, and the wound did very well. Before the operation, readings of the index were on the whole low, and small doses of tuberculin produced a satisfactory response. The patient improved to such an extent as to increase 2 stone in weight, and resumed arduous duties. He continued with small doses of tuberculin at two and three week intervals. Whilst feeling thus well, the back of his right hand began to swell, the skin became reddened and ultimately broke down, leaving a typical tubercular ulcer, circular, and $\frac{1}{2}$ inch across. Unhealthy skin and subcutaneous tissue were removed, the hand was put at rest, and healing occurred in about two months. So far as opsonic indications have gone, tuberculin seemed to produce satisfactory responses,

but as the patient lives in the country it has been impossible to be at all systematic. It is possible that more frequent examinations may have afforded data which would have directed treatment to a more satisfactory result. Later still, whilst continuing quite small doses of tuberculin, the patient, though in good general health and activity, developed inflammation in the remaining testis. Two ounces of clear fluid were withdrawn from the tunica with a measure of relief, but ultimately, a small abscess opened, which left a sinus now practically closed. I cannot say that this is a very encouraging case for the unguided use of tuberculin. It must be admitted, however, that the patient has continued on active duty all the time, with the exception of two intervals of about ten days each, and has said that he feels in better health since receiving tuberculin. And this statement is frequently volunteered by patients receiving tuberculin.

This case suggests the principle borne out by others that, when the amount of chronic tubercular deposit is large, it is only safe, in the absence of sufficient index estimation, to use minimal doses of vaccine.

As a contrast to the last case, one may quote the following:—A male patient, now about 38 years, gives a long history of tubercular infection, including a tubercular hip joint in childhood. During the last three years, the trouble has been genito-urinary. There was perinephritic abscess opened and drained, leaving ultimately an unhealed sinus. There were present long continued symptoms of cystitis and bacilli in the urine. At the inception of the present treatment, the patient presented himself because of retention of urine. This was found to be due to prostatic and periurethral inflammation, purulent in part. Stricture of the urethra was also present, and was probably due to the tubercular process, as the patient had not had gonorrhœa. His weight was much reduced, and his general condition wretched. Under an anæsthetic, the tubercular pus was evacuated through a perinæal incision. A few days later, when his condition had improved, the urethra was progressively dilated until full-sized instruments may be passed, and the tendency to contraction is insignificant. Free drainage having thus been procured, and whilst the patient was still in bed, tuberculin inoculations were proceeded with, and it was found that the opsonic response was most satisfactory. At so long as ten days after an injection, the index gave such values as 1.1 or 1.2 time after time. The patient took an effective interest in his treatment, and followed carefully instructions as to rest. He went to his home in the country, where his doctor for a few months attended to the mechanics of the urethra, and he came to town about once a fortnight for index readings and tuberculin injections. The readings invariably proved above normal, and his progress was very satisfactory. He gained over 2 stone in weight and improved much in colour. Locally, the lumbar sinus healed, as has an old-standing perinæal fistula and with it the incisions made at the last operation. The patient has for some months resumed his ordinary duties, and has quite recently added to these a position of responsibility.

Of course, in considering the merits of any one case in a disease so variable as tuberculosis, it is difficult to say with certainty what would have been the course of the disease had tuberculin injections been withheld. Nevertheless, the impression formed by all those associated with the case was that the outlook was exceedingly grave, yet within a year the patient has apparently taken an absolutely new lease of life.

Equally satisfactory is a similar case of genital tuberculosis, with epididymal deposit, perinæal sinus, and abscess in the appendical region,

tubercular in origin. A history of some years duration with one or another of the foci active from time to time. Tuberculin inoculations over a period of six months resulted in an arrest of the activity with healing of all sinuses and a very marked increase in weight and well-being.

Recently a case of genito-urinary tuberculosis in a man of 35 years has returned to report. Two years ago, he had arrived slowly at the stage of very frequent micturition, occasional hæmaturia, constant pain and tenderness, much increased by walking or jolting. He had very turbid urine, albuminous and containing many bacilli. There were bilateral epididymal deposits of great extent, with adherent scrotum; the prostate was enlarged and tender, and both seminal vesicles were enlarged, boggy and tuberculous. At the present time, after two years' treatment with injections of T.R. 1-1000 mg. every fortnight, he returns to say that he is much better. In detail, this means that his colour has improved, and his weight 10 pounds to the good; that the epididymis on each side has diminished in size, though both are still as large as small walnuts, are no longer tender, and are very hard; that the prostate can now be handled freely, and is firm, with a distinct nodule on the right side, and that the vesicles are now firm and cord-like, though still easily palpable. He is able to hold urine for three or four hours, and says that all bladder pain and discomfort have vanished. The urine has become much clearer, but still contains a few pus cells, and scanty bacilli may be found by centrifugalization. This case was obviously on the down grade two years ago. The only treatment for the past two years has been the giving of sandalwood oil by the mouth, and small doses of tuberculin hypodermically. He has attended as well as he could to diet and hygiene, but resides in a damp climate, and has continued at mining work as well as the conditions allowed. There has been universal arrest of all the foci of disease, yet it is practically certain that living bacilli still reside in the epididymes, prostate and vesicles, and some small lesion occasionally sheds bacilli into the urine. With well-established advanced tuberculosis, such as was present in this case, one has to remain satisfied, for a long time at least, with some such compromise. The patient can be restored to a condition of fair general health; his foci of disease are in a state of siege, but the ultimate surrender is long postponed, and the garrison awaits patiently until relaxed vigilance may permit of an occasional sortie, more or less successful; and the end of the war can never be figured with certainty until every garrison has unconditionally surrendered. Until this has occurred, these patients are in effect tuberculosis carriers, distributing bacilli as others do typhoid, though apparently undisturbed in general health, raised to a fairly high standard by the use of tuberculin.

Again, a man of 30 years, of fine physique, non-gonorrhœal, noticed his left testicle swelling; frequency of micturition supervened, and he passed some blood *per urethram*. The urine contained tubercle bacilli. There was a small implication also of the right epididymis. The left testis became progressively involved, ultimately disorganized, and finally was removed. A few weeks later a small sinus formed over the right epididymis. Very small tuberculin injections brought about progressive improvement. The bladder symptoms have entirely disappeared. The left-sided wound has healed, the right one is a pin point sinus practically without discharge. It is nine months since the beginning of treatment. The patient has been in full work for four months, and is surprisingly well in appearance, being ruddy and within a pound or two of 15 stone

weight. He still undergoes tuberculin inoculations every three or four weeks, and his opsonic index remains satisfactorily high. Here, again, it is difficult to assign values to each element in the treatment. The disease in his case is certainly non-progressive. It is practically arrested. His general health is now superb. The check has been most certainly given to the disease, but when will it be mated? How long must this rugged state of health be maintained before all the tubercular deposit has become inert scar tissue? If our ideas on the subject be correct, it must certainly be advantageous to have a patient's tuberculo-opsonic index on the average high. But this alone will not prevent, say, one portion of a tubercular area from being bathed in fluids of low opsonic value at one time or another. This portion might therefore become for a time progressive, and if the balance of power be quite upset in this part of the body, it is reasonable to suppose that the tubercular process could here evolve to the stage of softening and abscess, despite the fact that the general blood stream were rich in opsonins. The possibility of a local reaction after even small doses of tuberculin, has a most important bearing on this phase of the subject.

I do not think there is much doubt that even the small doses we now use do produce local reactions; but they are as a rule small, in the first place, because the injection is small, and in the second place, because the positive opsonic phase so soon arrives and discourages to a great extent the spread of the process. With all our care, it seems therefore somewhat of a hazard as to whether the stirring up of a focus will be followed by local softening and abscess. These considerations go to show that it will never be wise to exceed doses of tuberculin of a certain small size, and great as the *à priori* tendency may be to increase the dosage, one's work and results have always urged the adoption of infinitesimal doses. In this connection, the further consideration arises as to whether it is better to continue tuberculin injections over a long period at regular intervals, or whether it would not be as well to exalt the index as much as may be by a short series of injections, and then to avoid the possibility of further stirring up of the focus by discontinuing them for a period during which hygiene and increased opsonin have full play. There is a certain amount of evidence to hand in favour of the latter procedure.

In continuing an account of cases, the next for consideration is that of a female patient of 25 years, who, four years ago, began to suffer increased frequency of micturition and hæmaturia, loss of weight and extreme languor. The right kidney was slightly enlarged, and there were tubercle bacilli in the urine. She went to England, where the right kidney was removed and tuberculin exhibited in large doses, as was then in vogue. She visited various health resorts, and ultimately returned to Australia, in a very enfeebled condition. A *régime* of complete rest, open air, milk diet, and very small doses of T.R. was entered upon. This case has been under observation for two years, and has improved very much in general health. She is able to go about in the ordinary way, though too long hours of activity prove fatiguing. Occasionally, a clump of bacilli may be found in the urine, and there is still slight nocturnal frequency of micturition, but the normal weight and colour has been restored. A careful watch of the index has been made throughout. Without tuberculin in the early days, the index had a tendency to remain at or about .8. It could be raised by small doses of tuberculin to a point or two above normal, but showed a tendency to a rather early return to former level. As time

went on, this was not so, and it was usually found at or slightly above normal at the end of ten days. The notes show on several dates temporary periods of lowered index .7 or thereabouts. These were always associated with sensations of unusual tiredness, and were met by increase of rest, and a diminution or temporary cessation of tuberculin.

Here the index was a ready indicator, and proved invaluable. Quite recently in this case I began the use of bacilli-emulsion, and found that the dose I used was too large. At any rate, the readings showed an index of .7 at a time when it should have been above normal. A diminution of the dose procured better results. The patient is at present out of town on holiday, tuberculin being for a time purposely withheld.

In a review of this case, one feels that the index has been a distinct guide to treatment on several occasions, and has, moreover, afforded one an opportunity of following the case intelligently. I find it hard to believe that so much improvement in local symptoms and general health could have arisen without the use of tuberculin. Still, having in view the occasional scanty presence of bacilli in a non-albuminous clear urine, cure cannot be said to have been arrived at.

There is a class of case that seems to be very obstinate. I refer to some of those of deep-seated bone tubercle about the pelvis and lumbar spine. I have had to do with three of such cases, and though improvement in general health and local condition has taken place, so marked for a time as to raise the highest hopes, recurrence in no mistaken way has occurred, and beyond doubt due to deep-seated caries or necrosis. Our procedure in these difficult cases has been to determinedly attack the deep-seated origin by surgery, until as free a drainage as may be has been established, and then to maintain free drainage for longer than appears necessary from the amount of discharge, the meanwhile exalting the index to tubercle by the smallest doses of tuberculin, and to any other pathogenic organism present by the appropriate vaccine. Some of the want of success in these cases may have been due to the unwillingness or inability present in all to take rest for a sufficient period.

Of urinary cases in which the bladder symptoms have been most prominent, I have been able to watch three closely. The first was that of a man of 40 years, who for two years past had experienced slowly progressive bladder discomfort and pain on jolting, frequency of micturition, turbid urine, and occasional hæmaturia. He came with a diagnosis of Bright's disease; but when seen he presented no evidence of renal involvement. The cystoscope revealed a few circumscribed inflammatory splashes of colour of early tuberculosis of the bladder. The tuberculo-opsonic index was .6. The patient was persuaded to stay abed for some weeks, and received a series of injections of tuberculin. His response to these was most satisfactory, and he was relieved of the worst of his symptoms in three months. At the end of a year, he reports that he goes six hours at night without passing urine, which is now always clear. He feels very well, and has resumed full work about his farm. The second case is that of a boy of fifteen years, who for nine months previously had suffered from increasing bladder discomfort and pain, and marked nocturnal frequency. The urine was always turbid, and hæmaturia occasional. It contained many pus cells and tubercle bacilli. There was no evidence of tubercle anywhere else. This case intelligently followed out a *régime* of complete rest, open air, and milk diet, for six months. Small doses of tuberculin were given every ten days. At the end of six months, there was a gradual return to a

more active life. Finally, he accepted a position involving considerable activity, and has kept it for the last six months. Since the beginning of treatment he has put on 20 pounds weight, and 2 inches in height. He holds water for four or five hours. The urine is now clear, but tubercle bacilli are still present. The third case six years ago contracted typhoid fever, which was followed by pelvic thrombosis. There developed a large knot of veins in the hypogastric region. Insidiously upon this followed increased frequency of micturition, bladder discomfort and turbidity of urine. He gives a history of various treatments apparently directed against a cystitis following upon bladder cyanosis, as suggested by the huge veins above the pubes. Ultimately, tubercle bacilli were found in the urine, and he came to Melbourne for treatment. For some months, he was subjected to the adverse influence of insecure employment and other disadvantages, such as inability to rest. The cystoscope disclosed a deep ragged ulcer over an inch across, near the right ureteric meatus. There is a small quiescent nodule in the right epididymis. This patient during the past year has gone about his work unceasingly, and has had none of the advantages of good hygiene. He has received small doses of tuberculin and a vaccine of streptococcus. He has had occasional courses of sandalwood oil by the mouth. He has gained some weight, being now about 12 stone—height, 5 ft. 7 in.—and is in very good general condition and colour. He still has nocturnal frequency, but less than formerly. He can hold water until the bladder contains nine ounces, and the intervals of micturition are increasing. I think the ulcer is much smaller and cleaner. The urine, however, still has tubercle bacilli, though it is never so turbid as formerly. I think this patient is getting better, though the very nature of the case and the conditions will make it a long one. The other cases of bladder tubercle under my care are not of sufficiently long treatment; but I think that in tuberculin, properly given, we have a remedy which in most cases will stay the otherwise usually downward course of the disease. That there is an amelioration of symptoms seems certain; but the question as to how many would be arrested and ultimately cured rests largely on concomitant circumstances, such as the presence of tubercle elsewhere, the absence of necessary rest over a long period, combined with correct hygiene and diet. * Some of these adverse influences render cure impossible.

Of cases of tubercular arthritis, a series of three knee joints form an interesting group.

The first was that of a girl of sixteen, who hurt her knee by falling, when alighting from a train. Neglect of advice given, and inadequate rest resulted, in a few months, in a very unsatisfactory joint apparently requiring radical operation. She was in low general health. An anæsthetic was given with the view to the performance of arthrodesis at least, but at the last minute, it was decided to give the then new tuberculin treatment a trial. Fixation was proceeded with, and insistence on correct hygiene enforced. Small doses of tuberculin produced most satisfactory rises in the index, and the patient's condition promptly improved. In six months, all swelling had disappeared, and the normal joint contour was restored. Fixation was not dispensed with until after twenty months from beginning of treatment. The patient has latterly received tuberculin once a month. The index at the end of the month readings was always between 1.1 and 1.2. During the treatment, the patient has reached puberty in a normal healthy way, and has improved wonderfully in weight, height, colour, and general nutrition. She has a movable useful joint,

easily giving half flexion. Use produces no change in the index now, whereas in the early days, some readings following manipulation of the joint showed the wildest fluctuations.

In a case such as this it is, of course, impossible to prove that any of the good result is due to the tuberculin. That could only be done by observations on two parallel series of many cases treated with and without the remedy.

The next case is that of a man of 22 years, who injured his knee three years ago. He was treated for simple synovitis in various ways, and later for rheumatoid arthritis, but got progressively worse, and when seen, presented a knee joint undoubtedly in an advanced state of tuberculosis. The circumference was 4 inches greater than on the sound side. Three fluctuant pockets could be felt around the joint. An X-Ray photograph showed no deep-seated bone focus, though the articular contours were hazy and ill defined. Anything like a radical operation was refused by the patient, so, under a short anæsthesia, the pockets were evacuated through small incisions and the joint fixed. The patient's general condition was very poor. He has been able and willing during the last year to carry out suitable rest and hygiene, and has done markedly well. He has become ruddy and increased 2 stone in weight. Locally, the change has been most gratifying. The knee joint has lost its flexed position, and has regained a normal bony contour. The incisions after weeping slightly for a few weeks, have closed up. There has been no sort of discomfort for several months, but the splints are still retained, the patient getting about with the aid of crutches. This patient's tuberculo-opsonic index was raised very satisfactorily by small doses of tuberculin. They were given every ten days for a time, but latterly he receives an injection every three weeks. There is every indication that he will make a good recovery with a movable joint. I think that in both of these cases irremediable joint trouble was imminent. They form striking examples of good results to be obtained by conservative surgery in conjunction with tuberculin.

Of course, as I say, I cannot prove that the good was in part due to the tuberculin, because of the vagaries of the disease, but my conviction is a very strong one.

The third case of knee joint tubercle was seen much earlier in its course. At the end of three months the patient began to improve in general condition, and now, nine months from the beginning, she is nearly 2 stone heavier, and free of all joint swelling and discomfort. Fixation is still in progress, but there is no doubt that she will ultimately have a perfect joint.

In turning to the consideration of lupus, one is struck with the fact that the cases fall into two groups—one in which tuberculin cures in a few months; the other which tuberculin arrests and diminishes the disease, but has not so far cured.

Thus, a female patient of 60 years had a patch of lupus on the right cheek for ten years. Various treatments, including scarification and caustics, had been employed with ill success. When seen, she presented an oval lupus ulceration, somewhat larger than a crown piece. The index was .7. Tuberculin inoculations raised the index to levels averaging 1.2. Healing began at once, and was almost complete in eight weeks. Finally, a healthy movable scar was left with no sign of active disease.

A patient of 68 years had suffered from lupus of the left nostril and ala nasi. For many years, she had undergone various scraping and cutting operations, but there had never been complete healing. She had also about a year and a half ago a series of applications of X-Ray, but ulceration still persisted. One lesion remained on the septum and one on the inner surface of what was left of the ala. Small doses of tuberculin for three months resulted in complete healing, though improvement began at once. There is no doubt that the two are associated as cause and effect. I have formed the impression that elderly patients respond quickly to tuberculin.

The third case of complete cure was in a girl of eighteen years, who years ago had tubercular glands removed from the neck. A small patch of lupus appeared recently, one inch below the left lower eye-lid. It disappeared in two months under tuberculin. The foregoing three cases have every appearance of complete cure; but here again, it is difficult to be absolute.

For example, a female patient of 30 years was seen with a lupus plague of the size of a shilling on the right side of the nose. She presented atrophic scars from old lupus on the arms and legs, and there was some destruction of the right ala nasi. The fauces were somewhat deformed from the same cause, and there was evidence of mild intranasal disease also. Now this patient responded in a most satisfactory manner in index and clinical progress to tuberculin. She also frequently volunteered the statement that she felt infinitely better in general health as a result of tuberculin injections. In five months all activity of the disease had disappeared. The intervals between tuberculin inoculations were increased to a month, and later still, to six weeks. For a year the patient was apparently cured. Yet she returned quite recently with a typical patch on the other side of the nose bridge, and a bluish congestion extending from it to the tip. Calcium lactate internally produced no improvement, nor was there any evidence that chilblain was a factor. Now, the surprising part of the recurrence was that the index was at this time 1.2 apart from any tubercular injection. It is a striking example of a phase in the life history of tuberculosis. It seems that, despite the favorable conditions of the general blood stream, there can arise occasionally local conditions suitable for the development of further tubercular deposit, and that once this is established it takes much longer and very favorable conditions for the reverse process to be set in action and to bring about cure.

A striking example of how a grave case of facial lupus may be benefited by vaccine is the following: A patient of 40 years of age suffered for many years from advanced disease, and extensive ulceration involving both ears, cheeks, chin, and the whole bridge of the nose. Her condition was pitiable in the extreme. Before tuberculin régime, X-Rays were applied systematically, and produced considerable amelioration, but relapse occurred very rapidly. Two years ago tuberculin was commenced, and she proved very sensitive indeed to it. Quite small doses produced satisfactory responses, and they have never been increased. It soon became evident that all the ulcerated surfaces were about to heal, and they have remained so ever since. The ears and skin across the nose are stiff from scar tissue, and the chin presents a few verrucose spots probably of X-Ray origin. The patient is quite comfortable, and pursues her ordinary avocation unhindered. There is no doubt that the whole disease is quies-

cent, but one cannot say more than this, because on several occasions when she has had attacks of feverish epidemic catarrh, one or another of the affected areas has lighted up, becoming bluish red and somewhat elevated, but never ulcerating. This condition of practical stand-still quiescence has existed for nine months. The tuberculin is still being given.

Another case, equally as grave as the preceding, improved somewhat during the two months under treatment, but was unable to continue. The last case of lupus to be mentioned is that of a girl, who, three years ago, when fourteen years of age, fell and sustained gravel rash of the nose and upper lip. The abrasions did not heal, and took on the condition of typical lupus. At the beginning, she received large doses of tuberculin, which made her very ill and aggravated the lesions. Later, the small doses were instituted and produced satisfactory rises in the index, but progress has been slow. Before the treatment began, the whole of the tip of the nose was involved, including the alae, superficial ulceration and crust formation being present; lesions also existed on the gums and hard palate; several teeth have been removed. X-Rays have been considerably employed. The ultimate result has been that the patient has developed bodily in a vigorous normal manner, and has a good colour. The nasal lesions are in a condition of arrest bordering on cure; there has been very little loss of tissue. The buccal lesions have greatly improved, but one small ulcer a $\frac{1}{4}$ inch across still remains. A small dose of old tuberculin produced a more marked reaction, followed by healing, but relapse occurred shortly. I am now trying bovine tuberculin T.R. in the hope of finally relieving this obstinate case.

I have had one case of inoculation tuberculosis. A healthy man of 35 years cut his finger. Healing did not occur for four months. A lump appeared in the axilla. Mr. Bird operated and removed some large glands, and emptied a pocket of pus, in which I found tubercle bacilli. The patient's index just before operation was .9. Small doses of tuberculin were injected, giving good responses, *e.g.*, 1.5 in four days, 1.2 in ten days.

On removal of the gauze plugging, the wound healed very promptly, there being no tedious sinus left. Dressings were discarded in ten days. He had three more injections of tuberculin. I think the ready complete healing was hastened by tuberculin injection. Thus, it seems from all the foregoing that:—

1. The opsonic test of Wright, viewed as a laboratory investigation, is in competent hands accurate and reliable, in the same sense and to an equal degree that a blood count or a hæmoglobin estimation is accurate and reliable.
2. That as a diagnostic aid it is in many cases invaluable. That where the lesion can be stirred up mechanically to produce an auto-inoculation, its value is absolute, but that apart from this it fails sometimes because of too few readings being taken. In any case, like other clinical observations, it must be considered in conjunction with all other available data.
3. That as a guide to therapeutic measures, such as giving of tuberculin, the ordering of rest, and the resumption of activity, it is invaluable. In this connexion the more observations made the more intelligent the appreciation of the case in all its bearings.

4. That tuberculin inoculations are advisable in minimal doses at longer intervals, even when the opsonic guide is unavailable; but that the avoidance of errors will sometimes thus be impossible.
5. That the use of small doses of tubercle vaccine is a measure of prime importance.
6. That the test of Wright is a measure of the plasma resistance and pretends to be no more. The question as to whether it is a measure of the whole body resistance remains unsettled, though there is some evidence to show that cell resistance is a variable quantity.

FURTHER NOTE.

Since writing the foregoing paper, I have had the pleasure and advantage of listening to, and incidentally taking part in, the discussion on Tuberculosis at the recent Medical Congress held in Melbourne. I was there struck by the ready acceptance of the statement made by one contributor, that uniformly beneficial results followed from the treatment of the cases of tuberculosis by increasing doses of tuberculin, until doses were arrived at, at least 1,000 times as large as those mentioned above. I was much exercised in mind for an adequate explanation. Taking first the objections raised to the use of opsonic estimations: My own objections to it have been set forth very clearly, and are the results of several thousands of readings. They refer to the possibility of the index being only one of several measures of the variable body resistance, and also to the impossibility of taking sufficient readings to give a full and continuous representation of the clinical state of many cases in hand. But I still hold strongly that fewer readings have their value in diagnosis, prognosis, and treatment, in the fullest sense, and I shall continue to use them so.

But the objection was raised that it was impossible to get a suitable emulsion for the test. I have shown that this is not so. The objection was also raised that the index was often arrived at some time after the blood was drawn, and that by this time, even a day or two, the blood might be at another level altogether. To this, I say that my readings are always complete on the same day, and as a rule within three hours of commencing operations. Moreover, of the variation in index as occurring in a case such as suggested, this is precisely the condition sought after by further readings, and it is just in such cases of wide variation in index that the test is of such value in guidance to rest treatment and the avoidance of anything like a large dose of tuberculin or other deleterious influence. Objections to the test, such as these, are not objections *per se*, but only objections to the test as performed at the hands of the objectors.

I shall continue to use the test—

1. Because I am so placed as to be able to do so without undue labour, having skilled assistance.
2. Because it gives a larger view of tubercular cases.
3. Because it may finally divulge some secret at present withheld from us.
4. Because it is undoubtedly useful.

Referring to the larger doses of tuberculin, it cannot be denied that benefit unquestionable and permanent has arisen by such procedures. But if equally good, if not better, results accrue from the gentle method of Wright, and I hold that they do accrue, is it not better that the great

risks of local reaction following the large doses be avoided? That these risks are very real, the discredit into which Koch's original work fell, undoubtedly confirms. And it is here I think that the work of Wright deserves the highest praise. Whether one continues to use his test clinically or not, all must admit that bacteriotherapy of tuberculosis was placed on its first rational footing by his monumental work.

My own clinical observations have been made upon cases of surgical tuberculosis, and I can have no reasonable doubt that if tuberculin is to be used, it shall be in minute doses properly spaced. That it has produced a measure of benefit in some of the cases I am equally sure; but that we have arrived at anything like a sovereign remedy for all cases of tuberculosis is obviously untenable. The hope of cure is the brighter the earlier the case comes under treatment, and the better the conditions present; some cases, however, will go steadily downhill, whatever measures are adopted.

Finally, in drawing any conclusions about the treatment of tuberculosis, it is very essential to have the clearest appreciation of the vagaries of the natural history of the disease, with its occasional unaided miraculous improvements and apparent cures and, *per contra*, with its reappearances after long periods of quiescence.

ACTIVE IMMUNIZATION MEASURES OF TREATMENT IN PULMONARY TUBERCULOSIS.

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I propose to discuss, as briefly as possible, the application of recent research work to our present day treatment of pulmonary tuberculosis, with special reference to the employment of active immunization measures against the disease. In this particular method of treatment—as I need hardly remind you—an attempt is made to stimulate the production of protective bodies in the organism by injecting into it various tuberculins or other derivatives of the offending bacillus.

As introductory to the subject, I simply pass over, in a word, the sensational announcement of Koch in 1890; the anti-climax of disappointment which followed shortly thereafter; the practical abandonment of tuberculin for many years; the occasional good reports of a few enthusiasts who remained loyal to the remedy; the elaboration of new tuberculins by Koch and others; the considerable modification evolved both regarding dosage and selection of cases for treatment; the further studies on the immunizing mechanism; on mixed infection; the work of Wright and Douglas on the opsonic index; and the more frequent employment of tuberculin as a therapeutic agent—these are all landmarks in a controversy which even yet is not closed.

I wish to direct your attention first to the subjects of—

LABORATORY AND CLINICAL IMMUNITY.

It is, of course, well understood how much we owe to experimental evidence in working out the problems of immunity. Unfortunately, in the case of tuberculosis, the evidence is still very incomplete, and partial

as the work is I cannot attempt to do more than epitomize it here. I refer you to the work of Koch, Behring, Moeller, McFadyean, Trudeau, De Schweinitz,¹ Pearson, and others. The laboratory experiments of these authorities may, I think, be fairly summarized in the following words which Trudeau applies to his own work, in association with that of Baldwin, Kinghorn, and Allen:—

1. That dead tubercle bacilli increase, though to a very slight degree, an animals' resistance to subsequent inoculation.
2. That the living 'attenuated bacillus gives a stronger degree of immunity than the same bacillus killed by heat.
3. That the degree of attenuation of the bacillus used as a vaccine, bears a distinct relation to the degree of protection it affords in guinea pigs to subsequent inoculation with virulent human cultures.
4. Toxic immunity, or immunity brought about with dead germs is never as strong or as lasting as that produced through the medium of a living virus.

I also refer you to the work of Marmorek and Maragliano in their attempts to produce an anti-tuberculous serum. The conclusion drawn from the research of these and other workers, appears to warrant the statement that no absolute immunity has yet been produced against tuberculosis, but that a certain measure of success, amounting at least to an increased resistance against the disease, or what we might term a "relative immunity," has been obtained. Accepting this as a fair statement of the position, we now proceed to inquire how far these results of experimental research may be applied to the treatment of tuberculosis in the human subject. And it is, perhaps, opportune to mention that it is one thing to produce immunity against tuberculosis in an animal which is healthy at the commencement of inoculation; it is quite another and more difficult problem to produce immunity or cure in an animal already suffering from tuberculosis. In other words "Laboratory Immunity" is one thing, and "Clinical Immunity" another. Further, certain active immunisation measures employed in the laboratory, *e.g.*, by the injection of living virulent, or even attenuated strains, of the bacillus, could not, of course, be allowable in the case of the human subject. Recognising then the fact that laboratory experiment has as yet only succeeded in producing a relative, and not an absolute immunity, and remembering further, that the most powerful methods of producing this partial immunity by living cultures are not available in the case of man, it is at once obvious that any artificial immunization of a patient by tuberculin products can only be successful up to a certain point. This seems to correspond with the clinical observation that one attack of pulmonary tuberculosis does not, so far as we know, protect against a recurrence of the disease at some future date. Nevertheless, it is possible that the artificial production of a relative immunity might be of great assistance, particularly in cases where the issues are doubtful. In such case the production of an increased resistance might suffice to turn the clinical balance in the direction of recovery. As a matter of fact, I believe such an occurrence may not infrequently be brought about by the injection of tuberculin products in sufficient doses, and at properly spaced intervals.

Having discussed the experimental evidence in favour of immunity, I now proceed to a consideration of the results of inoculation of tuberculin products in the human subject, who is already the victim of

tuberculous disease. The therapeutic inoculations date back, of course, to the introduction of Koch's original tuberculin in 1890, at a time when the nature of the product, and of the immunizing process were both in a much more nebulous condition than they are at the present moment. Since that time various modifications of tuberculin have been introduced, such as the newer tuberculins T. R. and Bacillen Emulsion of Koch, the tuberculins of Klebs, V. Ruck, Denys, Beraneck, Behring, Spengler, and many others. It is unnecessary to describe these preparations in detail, but it may be useful to remember them as falling roughly into two groups—(1) Those containing the exotoxins or culture filtrates of the tubercle bacillus, such as the old tuberculin of Koch, and the tuberculin of Denys; (2) Those containing the body substance or extracts of the tubercle bacillus itself. We might add a third class containing a combination of both groups. All the tuberculins I have mentioned—both old and new—will, if given in proper doses, and at proper intervals, produce a rise in the immunization curve as measured by the opsonic index. This, however, does not necessarily indicate that the various tuberculins are of equal value; indeed, we have good reason to believe that various types of immunity may be produced according to the particular type of tuberculin administered. It is therefore advisable in selecting a tuberculin for clinical use to have some objective in one's mind as to the effect which one wishes to produce. An anti-toxic immunity, for instance, may be useful in certain cases, although according to modern theory any such immunity is not so lasting as anti-bacterial immunity brought about by the somatic substance of the bacillus itself. I shall return to these points later in my paper, but meanwhile we may say that by the injection of various tuberculins in the human subject we can stimulate the production of different protective bodies, the most important among these being ones possessed of anti-toxic, opsonic, and agglutinating properties. The recent literature of the subject, especially in English-speaking countries, is so full of references to the opsonic index as a guide to treatment in the use of tuberculin, that it would appear necessary on an occasion like this to pay the subject more than a passing notice. I do so with the more pleasure since the work of Professor Wright and his pupils has thrown a flood of new light on the immunizing process, and has helped to place on a more scientific basis a treatment which some of us have been particularly interested in for many years, namely, the Sanatorium treatment of pulmonary tuberculosis. I need hardly remind you that the three cardinal principles of treatment in this disease, according to the clinical methods evolved in the Sanatorium during the past fifty years are: (1) a constant life in the open air, (2), a generous dietary, and (3) strict regulation of rest and exercise according to the indications of each individual case. The absolute rest in the pyrexial stages of the disease, the value in non-febrile cases of prescribed exercise beginning with a gentle stroll on the level, and later the graduated hill climbing and other more severe exercises evolved by the clinical acumen of men like Brehmer, Walther, and later, Paterson, have received in recent years a scientific and rational explanation in the auto-inoculation records shown by Wright's opsonic index. In this way, by a series of clinical gradations, the patient received just the requisite auto-inoculation from his own tubercular focus, until finally the lesion is encapsuled, and the most severe exercise fails to displace the opsonic index beyond normal limits. The arrest or clinical cure of pulmonary consumption by natural and rational methods has been admitted for many years; it is only now that we begin to recognise some of the intricate mechanism by which that favorable result is actually accomplished.

Having paid this due tribute to the author of a most brilliant hypothesis, I hope it will not be considered ungracious if I fail to enrol myself in the ranks of those ardent disciples who in season and out of season, declaim on the value of the opsonic index as a necessary guide in the practical everyday work of inoculation. In the following remarks I confine myself principally to the inoculation of tuberculin in pulmonary tuberculosis, of which I have some little knowledge, and in which I am satisfied good results can be obtained quite independent of those frequent estimations of the opsonic index recommended by Wright and his pupils. I do not propose to base my criticism on the possible errors of technique in estimating the index, on the difficulty of obtaining a uniform emulsion, of excluding absolutely the occurrence of spontaneous phagocytosis, of taking as the basis of comparison a so-called normal serum which itself varies even from day to day. I am willing to admit for purposes of this discussion that with reasonable care and practice, the estimation can be made with a small percentage of error. But the further objection holds that even if a correct index be estimated, this index is not that of the blood at the time of inoculation, but of a sample taken some hours previously. This objection is not, perhaps, a serious one in many cases of local tuberculosis where an approach to absolute fixity can be obtained, but in the case of pulmonary tubercle it not infrequently happens that an oscillation of the index will take place in the course of a very few hours. For it has to be remembered that the lungs cannot be put in splints; to and fro movements are necessary for respiration; and movement, as we know, not infrequently spells auto-inoculation. A fit of coughing may produce an auto-inoculation with the greatest ease. I am of opinion that if the index is to be of much practical value in pulmonary tuberculosis it will be necessary to modify the process, particularly in two directions: (1) by the provision of an absolutely standard serum, (2) by such a simplification of the process that the index can be estimated within a few minutes of inoculation. The possibilities of error I have mentioned do not detract from the fact that the opsonic index, even if only approximately correct, still offers us valuable guidance in the course of the disease in many cases, guidance which I, in common with my colleagues, am glad to make use of as occasion arises. But I cannot help thinking that the opsonic process after all gives us but a partial picture of the process of immunity, and I am unable to accept its guidance absolutely as the be all and end all of tuberculin treatment. On this point I would remind you of the claims of Wright and his pupils that the opsonic index is our sole safe guide to inoculation, and that danger and disaster will almost certainly follow a disregard of this rule. I myself am old-fashioned enough to believe that the pulse, the temperature, the physical signs, the composite clinical picture of the disease as estimated by the older physicians, are still of value, and that these cannot yet be altogether replaced by the mechanical ingenuity of the man in the laboratory. When, therefore, we find the clinical symptoms in conflict with the opsonic index, some of us are not always prepared to accept the dictum of Allen, who, for instance, says:—

In the present state of our knowledge the index remains the only safe guide; pulse, temperature, and even clinical appearance, afford no indication of the proper time for injection.

Time will not permit me to multiply instances, but it is already well recognised that in spite of an apparently favorable opsonic index the lungs may fail to improve; or again conversely that a distinct improvement in physical signs may take place without a corresponding improvement in the opsonic index.

Let us further consider the relationship of the opsonic curve to the other curves of immunisation. Of the various protective bodies in the blood, Wright and his pupils regard the opsonins (assuming that they are specific bodies at all, which is doubtful) as the most important in the group. In regard to the particular disease of tuberculosis, I am not at all satisfied that Wright or any of his pupils have proved their claim. It has been held by Koch and his school, for instance, that the really important protective bodies are the agglutinins. Now, while simultaneous opsonic and agglutinative curves have been worked out by a few observers, the final inferences are anything but satisfactory. Sometimes the curves run parallel, at other times there is no relationship between the two. Baldwin, Kinghorn, and Allen have carried out some experiments on the serum of immunized rabbits, and it is interesting in this regard to note that "No coincidence was noted in our experiments between the agglutination of Koch's emulsion and opsonic power, although the phagocytosis was generally more complete when the serum agglutinated the bacilli." Returning to the results of inoculation in the human subject, Koch has, with the help of his tuberculin bacillen emulsion, succeeded in raising the agglutinative properties of the blood to as high as 1 in 300; Spengler, with a tuberculin preparation of bovine source, has raised the agglutinating properties to as high as 1 in 3,000. In the case of opsonins the result of one injection on top of another is only exceptionally cumulative; in other words, each injection of tuberculin stands by itself, so that in the words of one of Wright's pupils "it is almost impossible to raise the index and *maintain* it at a high level." When the high tide index drops to the low tide opsonic index another injection is given, and so long as the injection is just sufficient to raise the opsonic curve satisfactorily, Wright says the dose must not be increased, and that, as a matter of fact, such a progressive increase of dosage must inevitably culminate in disaster. Compare this routine with the German school who deliberately increase their doses to such an extent as to produce local hyperæmia and toleration to tuberculin in large doses. By this method a cumulative increase in various protective bodies, such as the agglutinins, may be brought about. Assuming that the latter are of some importance in immunization, it would appear possibly more satisfactory to follow a dosage which will increase your protective agglutinins to several hundredfold the normal quantity, rather than to be content with opsonic curves which do not cumulate and which seldom increase beyond twice the normal, taking 1 as unity.

Before leaving this part of the subject I would remind you that progressive dosage is the usual method adopted in the production of experimental immunity. For instance, if we wish to produce an anti-diphtheritic serum of high anti-toxic content, it appears necessary to inject the horse with increasing or progressive doses of the diphtheritic virus. Similarly, if we wish to protect guinea pigs experimentally, so that they may resist artificial infection fatal to controls, then it is the custom to inoculate them with fairly large doses of tubercle culture. So far as I am aware, though on this point I am open to correction, neither Wright nor any of his pupils have carried out the crucial experiment of immunizing animals to such a degree by means of the infinitesimally small doses which they employ. Whatever the rationale of the process, it is by this method, namely, progressive dosage applied to clinical work, that Moeller, Trudeau and others have in pulmonary tuberculosis produced results brilliant in comparison with all their previous efforts: results in plain contradiction to the teaching of Wright, who asserts that such dosage must inevitably culminate in disaster. I leave it to others to explain this apparent discrepancy.

I have already mentioned the agglutinins and opsonins, but in addition to these there are, without doubt, other protective bodies, including the anti-toxins, which play a part in the immunizing process. How important these anti-toxins are relatively to the other protective bodies, we do not yet know for certain, and there are authorities who hold that in tuberculosis the anti-toxins play an even higher rôle than the anti-bactericidal elements. I am personally quite satisfied from clinical observation that in certain cases of tuberculosis the old-fashioned culture products containing the exotoxins will not infrequently succeed when all other methods have failed. I will mention one instance only, since time will not permit me to develop the subject from the clinical aspect. Two years ago I had a patient with advanced progressive disease in both lungs, he had also tuberculosis of one testicle and of the right vesicula seminalis, upon which the surgeon to whom I referred him naturally refused to operate. To complete the picture, he had tubercular infiltration of both vocal cords. Several months' rest in bed failed to produce improvement, or to make any marked difference in his pyrexial condition. Although the opsonic indices were somewhat fluctuating and contra-indicated injection, I ventured to give him several tentative injections of tuberculin T. R., with the result that he lost weight and failed to improve. I then tried Bacillen Emulsion, and again he failed to improve, and the loss of weight continued. Although I had not much hope of success I next tried him with old tuberculin, beginning with 1/20,000 c.c., and gradually increasing the doses. The patient immediately began to pick up, put on weight regularly each week, and in a few months was discharged, able to return to work. Although the disease was not absolutely arrested, the patient has been in regular employment in Sydney for the past eighteen months, and last time I saw him was riding a bicycle on his daily round in pursuit of business. Occasional cases such as these make one feel that our knowledge of immunity reactions is still far from complete.

The work on opsonins has added another facet, and a most valuable one at that, to our knowledge, but, after all, I am confident it only gives us a partial view of the immunization process, and for the reasons given I cannot blindly follow those who declare it to be the sole safe guide to inoculation treatment. I think it very possible that in the near future we may be able to discover that certain tuberculous patients are weak in response to one or other protective body in particular; that in one, for instance, we must stimulate the opsonins, in another the agglutinins, in another the anti-toxins; and it may be necessary to employ one or other particular type of tuberculin and dosage according to the particular result which we desire to effect in the individual case.

I intended to discuss finally the clinical deductions drawn from a practical study of tuberculin inoculations in a large number of cases under my care for the past few years, but time will not permit. I will, therefore, content myself with summarizing briefly the conclusions at which I have arrived:—

1. The principles of sanatorium treatment—open air, generous diet, and strict regulation of rest and exercise according to the indications of each individual case—still constitute our main basis of treatment in pulmonary tuberculosis. No process of immunization at present known to us can displace this rational hygienic treatment.
2. Experimental and theoretical considerations on immunity show that properly spaced injections of tuberculin may raise the resistance of the tissues against the disease, but cannot produce an absolute immunity.

3. Such increased resistance may be of use (a) for therapeutic purposes during the process of healing; (b) in helping to prevent a recurrence of the disease at a later date.
4. No constitutional immunizing process is of much use in pulmonary tubercle unless it is accompanied by local changes in the direction of (a) absorption of recently formed tubercle wherever possible; (b) the formation of a firm cicatrix around the focus of degenerate tubercle.
5. In the production of a cicatrix around the lesion the local hyperæmia reaction produced by the injection of tuberculin is possibly of more value than any constitutional immunity which the inoculations have as yet been able to produce.
6. So far as I have been able to test the subject in actual practice over a large series of cases, perfectly good results can be obtained in pulmonary tuberculosis without the help of the opsonic index. In this respect my results are in complete accord with the views of Trudeau, R. W. Philip, and other recognised clinical authorities.
7. In regard to progressive increase of dosage *versus* the infinitesimal doses recommended by Wright, I have made it my practice to begin with the latter doses in all cases; but my observations lead me to the conclusion that in many obstinate and difficult cases a progressive increase in dosage will still further enhance our results.

The suggestions of disaster by Wright and his pupils in these cases are, so far at least as pulmonary tuberculosis is concerned, disproved by the actual results of practice in the hands of competent observers.

VALUE OF OPSONIC ESTIMATIONS AND THE USE OF TUBERCULIN IN PRACTICE.

H. C. MILLS, M.D., (Sydney).

There is still a considerable amount of prejudice against the use of tuberculin in the treatment of tuberculosis. It is looked upon by many as a substance introduced into the body of the sufferer from tuberculosis. But this is hardly the case. The sufferer from tuberculosis, unless he has attained a considerable degree of immunity, nay even then, is daily and hourly receiving into his blood stream the tuberculin, the product of the bacilli already in his system. He is vaccinating or inoculating himself, *i.e.*, he is the victim of an auto-inoculation as described by Wright, and it is only as a result of this inoculation that the sufferer works out his own salvation. The same general principles are at work in bringing about the recovery of the tubercular patient as obtain in the case of other infective diseases. Paradoxical though it may seem, the statement may be held as correct, that the tubercular infective agent which brings about the disease, is also the means of bringing about the cure. Without the tubercle bacillus there can be no tuberculosis, and in the tuberculous, without the aid of the bacillus, there cannot be brought into action the protective mechanism of the body, which, when sufficient, ultimately leads

to the arrest and cure of the disease. When we speak of resistance to or immunity against a disease, we mean, I take it, that the organism is capable of producing sufficient protective substances, be they bacteriolysins, agglutinins, opsonins, &c., to prevent the further growth of the causal agent of that disease, or to neutralize its injurious toxic effects. Were it otherwise, we should all become victims of the disease—no response on the part of the defensive mechanism of the body, no immunity, no resistance.

Further recovery depends upon the readiness by which the protective agents are called into activity. This in turn depends upon the general state of nutrition of the individual. Hence the great efforts made in sanatoria to nourish the body to the fullest extent, to bring it so to speak up to concert pitch by full feeding, fresh air, rest, and exercise. It is, I know, generally lost sight of that the means employed at sanatoria are not in themselves curative. The laity and many members of the public think that a gain in weight means a subsidence of the disease, that breathing in fresh air leads to the arrest of the disease—but the more one thinks about how a cure is brought about the more one is led to the conclusion that these sanatorial agents only induce a condition of improved general nutrition of the cells and fluids of the body, that with this improved state of nutrition the protective mechanism is made stronger, that as a consequence now of the stimulating influence of the invading micro-organism protective substances can now be furnished in increased amount and the disease by these direct means arrested or finally got rid of.

Chronic and Acute Types.—With this conception are we not able to understand why the disease is acute in some, and chronic in others? In the acute cases no, or quite insufficient, protective agents are formed, while in the chronic cases the disease is stayed for a time, then progresses again—stayed, for the protective bodies are sufficient, but insufficient during the period of progression.

Evolution of Immunity.—The direful effect produced by the *B. tuberculosis* when it attacks a race of people in whom previously it was unknown, shows that a state of immunity to the disease has not been developed. When one contrasts the effect of tuberculosis in the case of those people who have suffered from the disease for hundreds of years, with the effect produced in people such as the Australian aboriginals, one sees that in the former a resistance to the disease of no mean order has been developed. In the case of the latter, the disease simply runs riot—there is no resistance, practically no powers of immunity are present.

In the animal world too, we find a parallel state of affairs. The English Commission on Tuberculosis showed that animals varied very much in their powers of resistance to the effects of the *B. tuberculosis*. Whereas monkeys, so closely related to man, were very susceptible to the disease, and showed its effect in a very marked manner, yet rats and mice exhibited marked resistance to the disease. Now, rats and mice are far removed from man, but they have been closely associated with him for hundreds of years—living in the same home—eating his food, and subjected no doubt to the influence of the bacilli distributed by the infected.

All this means that a state of immunity with regard to tuberculosis has been brought about, and brought about too by the direct influence of the bacillus upon the tissues, &c. We must grant then that man has attained a certain degree of immunity, that he has evolved powers of resistance to the disease which in the beginning he did not possess. His protective mechanism is now more easily aroused into activity, and is not so readily exhausted.

Another way of looking at the matter is that man is gradually becoming tolerant to the disease. By repeated infection his tissues do not react, or only slightly. The toxic effects of the disease are not felt, or felt only slightly. Such a one may be likened then to the so-called "typhoid carrier," in whom the typhoid organism gives no evidence of the disease, though it be present in millions. In the case of the sufferer from tuberculosis of what one may term the chronic type, there are often no symptoms of the disease except those due to mechanical causes. He carries round with him his invading organism, ejecting it frequently, but feeling no ill effects from its presence. In truth he is a tuberculosis carrier.

Now what does all this indicate? It means, as I understand it, that man in general is very resistant to tuberculosis, that this resistance is due to a state of evolution of active immunity—that this state of immunity has been brought about by tubercle bacilli acting upon his tissues and that of his forefathers of generations ago. Were it not so most of us would not be here. I take it that most, nay all of us, have suffered in some period of our life from tuberculosis that we have recovered, and are now more or less immune. We are now in a condition similar to a man who has been vaccinated against typhoid—he is less liable to get the disease now—less liable, too, to fall a victim to its ravages should he become infected. And why? I think the only explanation is that his protective mechanism has been aroused into a state of tremendous activity. It is now like that of a hair trigger. Granted now that there be exposure to infection, his defensive mechanism is at once evoked. All his means of curing the disease are actively put forth. The invading organism is attacked at all sides before it has time to multiply—no quarter is given, and the invader is destroyed.

In the inoculation of sheep against anthrax, surely much the same process is at work. One can hardly believe that the cells and tissues of the vaccinated animal are so altered that they no longer afford a suitable soil for the anthrax bacillus. It is, I think, much more reasonable to suppose that now, after inoculation, the defensive forces, hitherto dormant, have been, so to speak, sensitized, so that when the animal is infected they are at once brought into action, and the invading organism has no chance of thriving. These ideas bring out the importance of attacking at an infective disease early. When it is possible to treat patients in the early stages of the disease, the results are always more favourable. In no disease is this exemplified better than in the case of tubercular disease. The earlier the sufferer from tuberculosis comes under treatment the better are the chances of complete recovery.

It has already been indicated that civilised man is more or less immune to the disease. If his powers are to be used to the best possible advantage they must be concentrated on the invading organism before it has multiplied to any extent. It is not always a question of virulence on the part of the infective agent. The question of numbers comes into consideration.

The suggestions hitherto put forward all centre on the subject "immunity." If we physicians are not to be immunizators, as Wright prophesied, we will, at any rate, be all the better if we look at the subject of disease from the point of view of immunization. Tuberculosis is no exception of this idea. To do any good to the tuberculous we must try to immunize him, nay that is all that we can do. To fill him up with tonics and cough medicines, to rely on a few drachms of cod liver oil is the veriest nonsense. No, we must in our battle with tuberculosis have

some idea of the ills that are wrought by the tubercle bacillus, and how these ills are endured and remedied, and what forces can be called into play to destroy the infective agent, or to render it harmless. This latter view must not be lost sight of. Do we always destroy the diphtheritic organism after an attack of diphtheria? On the contrary, does not the Klebs Loeffler bacillus remain perfectly harmless in the mouth for a considerable period afterwards, the patient being perhaps a diphtheria carrier?

Is it not the same with pneumonia and typhoid and scarlatina probably? Yes, to render the infective agent harmless, or comparatively so, is no small feat to accomplish, and that in many cases the physician can bring it about I have no doubt at all. A boy who, suffering from tuberculosis of the left upper lobe of his lung, with consolidation and softening, treated by tuberculin, recovers to such an extent that he no longer looks ill, but has gained weight and strength, that he loses his cough and expectoration and temperature and feelings of weariness. Such an one is apparently well, but soon after his lung shows signs of trouble, not merely dulness to indicate a past thickening and scarring, but râles over a considerable area. If the tubercular process has entirely ended, why do these signs remain? If it has not ended the patient feels no sign of this, he carries his burden, but it is not irksome nor wearisome, the disease is practically rendered harmless.

In many cases we can bring about such a condition of the bodily tissues that they no longer react to the toxic influence of the tubercular infective agent.

We can immunise against the toxic effects of the tubercular disease, even if we cannot actually destroy the causal agent. Just in the same way as man can immunise himself against the injurious effects of alcohol, another is brought about, as we all know, by the regular injection of gradually increasing doses.

Now, resting assured that immunity is the end result of the interaction of the infective agent with the fluids and tissues of the infected one, are we justified in employing some one of the various tuberculin preparations to help us? Then comes the question of how these preparations should be employed. At the present time we have for our guidance the method employed by Wright, *i.e.*, extremely small doses administered once in a week or ten days, and the administration governed by the opsonic index, and the method employed by most continental workers, followers of Koch in the domain of tuberculin therapy, which consists in the administration of tuberculin in rapidly increasing doses and at much more frequent intervals.

Wright and his followers hold that by a persistent and gentle stimulus, the immunizing mechanism responds and is not exhausted. He maintains this view by bringing forward evidence to show that the amount of protective substances present in the body fluids is increased after each small inoculation. Further, he showed that these protective substances disappeared or diminished immediately after inoculation. His work brought to light the fact that a substance existed in the blood which when brought into contact with micro-organisms so altered them that they became readily engulfed by the phagocytes of the body. To this substance he gave the name of opsonin. Without the presence of these opsonins in the blood the phagocytes engulfed or devoured none, or only very few of these organisms. Further, he brought evidence to show the specificity of opsonins. The blood, for example, of a person who has or is being

immunized against a staphylococcic infection has an increase in the amount of opsonins in the blood which will so influence the staphylococci that these will be readily eaten up when brought into contact with the phagocytes, but they have no action or only a slight one upon, let us say, the tubercle bacillus. Wright further worked out a plan for measuring the amount of opsonins present in the blood. He showed, too, that the increase of opsonins and the increase of agglutinins ran on parallel lines. He concluded from the result of his labours that by measuring the amount of opsonins in the blood one has a means of ascertaining the amount of immunity present. Working along these lines he showed that a temporary diminution or disappearance of the specific opsonins followed the administration of large doses of any vaccine—be it staphylococcic pneumococcal, or tubercle vaccine—that small doses not increased beyond a certain amount were all that were sufficient to keep up a constant increase of opsonins, and with them of other protective substances in the blood, and that to immunise a patient against an infective disease the immuniser should regulate his doses by accurate measurements of his opsonic content.

In plain words, without such a guidance the immuniser would be working in the dark—that he would often bring about a great diminution of the opsonins present by the administration of too large a dose of any vaccine producing what Wright terms a negative phase in contradistinction to the positive phase, which is the condition when the blood fluids are rich in opsonins.

Wright contends that to produce a negative phase is to render the patient liable to the advancement of the disease from which he is suffering—for during the period of the negative phase his protective substances are at an extremely low ebb, and these conditions may remain for some time, being dependent upon the amount of vaccine administered. Perhaps too much has been made of this negative phase—that it exists is unquestionable, but that one should have such an intolerable fear of producing it is not good. "Beware how ye practice immunisation lest ye produce a negative phase" should not be our sole or chief text. Were the negative phase so horribly direful in its results, the work of most Continental observers in tuberculin therapy, Petruschky, Spengler, Roepke, Turban, &c., with Trudeau, Pottenger, and others in America, could only have been lamentable. But, instead, we find that their results are good and encouraging to a degree. Yet by their methods of rapid dosage and large dosage how many negative phases must they have produced, and how profound must they have been. "By their fruits ye shall know them," and this may well be applied to their work and their results. Certainly these followers of Koch are only following out the plan generally if not always adopted, in immunizing the lower animals against an infective agent, *i.e.*, to begin with small doses and to go on increasing until the animal eventually fails to react to a dose many hundred times larger than the initial dose. Would any one doubt in such a case that the animal's condition of immunity was vastly higher than when the process started.

By tuberculin therapy, too, a condition of greatly enhanced immunity can be brought about so that a patient who may react slightly in the beginning with a dose of 1/1000 of a milligramme of T. R. may become so immunized that not even does the slightest reaction follow upon the administration of one or five or ten milligrammes.

Is not that patient now in a better state to resist the ravages of the tubercular process? His tissues will not react now to the toxic effects of the tuberculin derived from his own bacilli. They have become resistant. By following out Wright's plan, certainly a degree of immunity is produced, but months after the treatment has been persevered with the patient will react, and react strongly to a considerably larger dose. Further, it can be shown that with gradually increasing doses the amount of opsonins can be increased after each injection, and a high opsonic content can be maintained.

But does the knowledge of the opsonic content give us a true idea of the amount of immunity we are endeavouring to produce? Does an opsonic index of 2, say, which all will regard as fairly high, tell us that the patient's immunity is only twice that of a normal individual. Then what is to be said in the case, say, of a patient who starting with 1/1000 milligramme tuberculin, reaches in the course of the immunizing process, say, a dosage of one milligramme. His index, carefully observed all the time, maintains a satisfactory high level even up to 2 or more, but as he has received 1,000 times his initial dose without re-acting, is he not to be considered a much more highly immunized being than the figure 2 would indicate. Possibly his state of active immunity may not be higher, but surely his state of passive immunity is greater.

One further observation bearing upon the subject. We know that the number of leucocytes in the blood of one individual may not be the same as in that of another. With a leucocyte count of 6,000, say, a patient may have an index of 1.5. What is to be said, then, of a patient who, with a leucocyte count of 10,000, has an opsonic index of 1.2, let us say. This observation of Dodds has not received the attention which, I think, it deserves.

Well, gentlemen, I have carried on various vaccinal processes for the past two and a half years. In many of the cases I have regulated my dosage by the state of the opsonic index. In others I have disregarded it. Relying on clinical symptoms and signs, but always endeavouring to secure what I might term a reactionless result, *i.e.*, that there should be no marked disturbance of the pulse, or temperature, or general feeling of well-being of the patient after inoculation, I really think I have secured as good results without the knowledge of the opsonic content as with it. But certainly it is both grateful and comforting to know that the index is high, and maintaining a high level, while one is proceeding on a course of immunization. Were I asked to express an opinion as to the kind of cases likely to benefit most by tuberculin therapy, I would say that in glandular and genito urinary cases the best results are met with. Let me illustrate the remark by quoting a case. An old man, 55, was sent to me, said to be suffering from tuberculosis of urinary organs. I found him with tubercular disease of epididymis, of seminal vesicles, of bladder, and of his kidneys. He had, too, some emphysema and occasional attacks of bronchitis and asthma. He had much pus in his urine, and when the pus was removed the urine showed about 1-12 albumen. He had frequent micturition, about every half hour, with very great pain. In fact, he was an old wreck. To-day, some nine months after treatment, there is not a trace of pus in his urine, and only the faintest haze of albumen. He passes his urine four or five times in the 24 hours, and never has to get up at night. He is quite free from pain. His epididymis is

still much thickened, and no doubt therein are his enemies. Still he carries them about unconsciously, and he is no longer distributing tubercle bacilli with every act of micturition.

With such a result can one doubt the good that specific tuberculin therapy may accomplish. But let us remember that while in specific tuberculin therapy we have, in my opinion, the most powerful agent with which to fight the ravages of the tubercle bacilli, let us remember, I say, that we must not neglect to use all the means that have been found of proved worth, such as abundance of food, fresh air, rest, and judicious exercise. These agents should be employed in conjunction, whether the case be one of genito urinary, glandular, pulmonary, or visceral type.

NOTES ON THE TREATMENT OF PULMONARY TUBERCULOSIS BY TUBERCULIN WITH THE AID OF OPSONIC INDICES.

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Many years ago I made extensive use of tuberculin both old and new, but finally discarded it as useless or harmful. I now believe that the reason of failure was that I did not carefully select my cases, and that I used too large doses.

A little more than two years ago, I again tried new tuberculin with the assistance of opsonic indices, and though the time is too short to form definite conclusions my experience has been sufficiently encouraging to lead me to persevere in its use, and I believe that in a certain limited class of cases tuberculin is a most useful adjunct to the open-air treatment.

During the last two years, I have used tuberculin in eighteen pulmonary cases, and have made over 500 opsonic estimations.

In the first place, I would speak of the value of the opsonic index. There is no doubt the technique is difficult, and possible sources of error numerous; however expert one may become it involves an immense amount of time and labour, but I believe that the conclusions derived from it are scientifically correct and helpful in deciding the suitability of a case and the dose to be used. And although in future I do not intend to make the estimation as a routine all through the treatment of a case, I think it advisable, especially in pulmonary cases, to satisfy oneself that the index is persistently low before treatment is commenced, as only in such cases can improvement be expected. I do not begrudge the time and labour I have expended on the subject, as the experience gained has been invaluable.

I have never found the tuberculo-opsonic index below .5, and rarely above 1.5, so that the possible limits are narrow, and unless the results are accurate, they are useless. I have used the index as an aid to diagnosis in doubtful cases of tuberculosis, and when repeated readings have given an index approximating 1, the after history of the cases has always confirmed the non-tubercular nature of the disease, but I do not now use the index for diagnosis as Calmette's reaction is so much easier and quite as reliable, and as long as only one minimum of a $\frac{1}{2}$ per cent. solution is used, I do not think any local harm can be done.

I would sum up my conclusions:—

1. The estimation of the opsonic index is sufficiently correct for practical purposes, and though other elements which tend to immunity may exist of which it takes no cognisance, the opsonin is the most important, and the one which most actively responds to the tuberculin injection.
2. In cases of active tuberculosis in which excessive auto-inoculation is continually taking place, the index may vary anywhere between the extremes .5 to 1.5 from day to day, or even in a few hours, therefore it is of no value as a guide.
3. In no case should injections be given unless the disease is running a non-febrile course and the index persistently low, under .7.
4. In such cases small injections cause no harmful negative reaction, but invariably a definite positive phase reaching its maximum in from five to seven days, gradually falling to what it was before the injection was given, after ten days or more. Another injection should be given before the index falls below 1; after repeated injections the index does not fall so rapidly.
5. The best results are obtained with small doses 1/2000th milligram. (reckoning that 1 c.c. of tuberculin contains 10 milligram.).
6. That exercise, by causing auto-inoculation, profoundly influences the index, and that an injection should only be given when a patient is quiet, and that he should remain quiet some hours afterward.

THE VALUE OF TUBERCULIN.

It will be seen that its use must be confined to a limited number of cases.

When the disease is very early, confined to a limited area with little or no softening, I think it is unnecessary, as these patients get permanently well if they follow an open-air life. But where there is a tendency to relapse, or the disease, though limited, is more advanced with softening, I find that Tuberculin, in small doses, 1/2000 milligram., persisted in for a long time, is most helpful, hastening recovery and warding off relapses. But before using it, the patient's temperature must be normal for some time, and his Opsonic Index persistently low, the slightest rise of temperature must postpone an injection for several days. As a rule, when the disease is more advanced, or more than one focus exists, Tuberculin is of no value, or may do harm. But if the disease is running a non-febrile course and the index low, it may safely be tried, and I am sure cases thus treated have improved, though they had failed to do so under sanatorium treatment alone.

As to dosage, I began by using 1/1000th milligram., increasing up to 1/600, but I now use small doses. 1/2000th milligram. is the common dose, 1/1000th may be used in very favorable cases, for it must be remembered that the more severe the disease, the smaller must be the dose.

In answering the question as to whether a careful observation of temperature (oral or rectal) is sufficient guide in the use of Tuberculin, I would say most certainly not. In the course of a thousand injections, I

have only had a rise of temperature (generally half a degree) half-a-dozen times, but I believe harm can be done, revealed by index, with a much smaller dose than it takes to raise the temperature even by half a degree, in fact, I would expect such a rise as that to be accompanied by a well-marked hurtful negative phase. The temperature is not nearly a fine enough instrument, and in the absence of the Opsonic estimation, I would advise keeping to very small doses.

A Tuberculin injection should never be given within a week of the time a positive Calmette's reaction is obtained, as it is very likely to cause a troublesome exacerbation of the eye symptoms.

I am trying bovine T.R. in cases where patients have failed to respond to the human, but I have not had sufficient experience to form any opinion as to its value. I began by using Tuberculin dilutions from an india-rubber capped bottle, but, however careful I was, I found that the dilution became muddy, and I now only use what I put up in sealed glass capsules, using as a diluent normal saline and carbolic acid $\frac{1}{4}$ p/c. Capsules thus prepared will keep indefinitely, and no smarting or inconvenience of any kind will be occasioned at the seat of puncture.

I have also used Tuberculin in cases of Tubercular Adenitis, joint disease, early Spinal disease, and Genito-urinary disease, and, so long as the disease is limited, the results have been good. I have found Tubercular glands of the neck (proved tubercular by positive Calmette's reaction) clear up wonderfully. I feel confident that the field of surgery in tubercular cases will be narrowed, and that open-air treatment and Tuberculin, where applied early enough, will in most cases prevent the catastrophes which in the past surgery has been called to meet. Though Tuberculin may be of assistance in some pulmonary cases, I feel sure that it is in localized limited lesions, so long left principally to the tender mercies of surgery, that Tuberculin will win its greatest triumphs.



SANATORIUM TREATMENT OF PULMONARY TUBERCULOSIS.

F. J. DRAKE, M.B. (MELB.).

The question of sanatorium treatment and methods has been the subject of a good deal of keen criticism during the last four or five years, some authorities even going so far as to raise the question, "Is it worth while?"—to submit the patient to the discomforts, loss of time, and expense of a course of sanatorium treatment.

The dictum, "once tubercular always tubercular," has obtained a certain vogue. No doubt the fullest hopes raised by the early results of sanatorium methods have not been fulfilled, yet the results obtained by these methods are far in advance of the older methods of treatment.

The rôle of the sanatorium in the treatment of tuberculosis is only just now becoming defined as one link in the chain, but one of supreme importance, not because of the cures expected to take place during the stay of the patient, but because of its educative character as regards the degree to which exposure to pure air, under all kinds of weather conditions, can be carried with benefit; instruction in suitable

and economical modes of diet, and especially the regulation of rest and exercise which requires the nicest judgment on the part of the superintending practitioner. The patient thus learns thoroughly his powers of reaction to the many factors that conduce to recovery from this disease.

He is also taught the modes of preventing reinfection, so that by protecting himself he at the same time protects others. As a result of the statement so often made to patients that a stay in a sanatorium for three or four months will result in a complete restoration to health, the methods of the sanatorium have been somewhat discounted, with the accompanying disappointment to many patients, their relations and friends, and to the discouragement of other intending patients.

I am more and more impressed with the necessity of telling the patient, irrespective of the stage of the disease, that he will require to follow the essentials of sanatorium methods, viz.—living in a continuous supply of pure air, maintenance of his weight, and avoidance especially of fatigue for many months after the apparent arrest of the disease, perhaps for two or three years, and possibly all his life. Few realize how much can be done even for advanced cases when they are given a chance in a well-conducted sanatorium.

The work is certainly harder to both physician and patient, but the result is more gratifying, and worthy of the effort in many cases. As the result of a stay of a few months in a sanatorium the lives of nearly all can be prolonged, and in those in whom we procure an arrest of the disease a life of limited usefulness may be secured, even without a cure.

If the patient have a strong heart and a good digestion, he has a fair chance of arresting his disease.

Again, in comparing home treatment with sanatorium treatment, one cannot deny that many cases do uncommonly well in their own homes; but for the average patient a sanatorium offers the best chance of cure.

The character of the patient for home treatment has also to be considered. Immature and unformed characters will generally do better in sanatorium, while mature and sensible individuals often do well outside them. For the melancholic, the continuous association with the constant topics of expectoration, cough, temperature, bacilli, and the deaths, sanatorium life is unsuitable (Cornet). Fortunately, these cases are uncommon.

Another class of case is those suffering from incurable home sickness. This feeling of isolation from relatives is a great bar to recovery. Many patients are allowed to remain too long at home under indifferent conditions in belief that they are not sick enough to go to a sanatorium, and are often told so by their physician. This is a misconception of the functions of a sanatorium. The sanatorium is a place for the arrest of the disease, and no case is too early to begin the cure after a diagnosis is once made.

Even after "breaking down," and tubercle bacilli are found in the sputum, it is computed that most sanatoria secure a happy result in about 60 per cent.

Arrest is more certain and secure in a sanatorium and also more quickly attained, and although the first cost is greater I think it is more economical in the end. It is almost impossible to efficiently treat advanced cases in the home, even of the well-to-do.

Such complications as tuberculosis laryngitis, tubercular bladder and kidneys, have yielded to sanatorium methods. There are few patients who cannot indure the life of a sanatorium, and those who are unable to do this have little chance of arrest of the disease under any conditions.

The tubercular individual is very loth to admit that he is seriously ill, and if at home will persist in doing many unwise and harmful things. He will entertain and be entertained by his friends and relatives, and will live the usual unhygienic life of the ordinary individual.

Very few of his relatives or friends are willing to give up their usual mode of life to keep the patient in the way of physiological righteousness. He is usually too near his business, and is always in the way of temptation.

In a sanatorium, the close relation of patient and physician is especially conducive to recovery, and where a good result so often depends on strictly following out orders in apparently trivial details. It is the regularity and precision that counts for so much, and are only to be gained as a rule in a sanatorium.

Complications can be dealt with promptly and thoroughly as soon as they arise. Very few houses are constructed so as to meet the special requirements of the tubercular individual, with trained assistants and nurses to see that methods for the prevention of re-infection are thoroughly carried out. Many think that life at a sanatorium is depressing and dull. This is contrary to experience, and although he has his dark days, the whole tenor of the place is one of cheerfulness and hope. There is among the patients a mutual feeling of sympathy, and he has the moral support of his fellow sufferers.

It is truly said that the sanatorium is the place where all those measures which aid in bringing about a cure in tuberculosis can be applied most easily and most successfully, and where the patient can live under the best circumstances pertaining to a cure. There is another side to the picture. The greatest disadvantage of sanatorium life is the danger of producing a lot of incorrigibly lazy and introspective convalescents, who lose their power of applying themselves again to continuous work when they have regained their health.

No doubt, in the case of manual workers this will, to a great extent, be overcome by the system of graduated labour now being adopted in some sanatoria.

The importance of early diagnosis and early treatment cannot be exaggerated, and I would exhort the general practitioner to exhaust all the means of diagnosis in any suspected case. No doubt there are many sicknesses associated with mysterious rises of temperature, accompanied by what are called the symptoms of influenza, and which are really due to pulmonary tuberculosis.

Eve, *Practitioner*, September, 1908, says:—"In phthisis, whatever the method of treatment, success depends on early diagnosis." There is a fatal conception, very prevalent in the profession, that it is a sign of incompetence to be unable to diagnose phthisis in its earliest stage by physical signs. It cannot be too clearly recognised that the disease may be months old before the most carefully trained ear can detect its presence. Hence the vital importance of some form of tuberculin test. Let us hope that we have found a reliable test in the so-called Calmette reaction. By the time the patient reaches the sanatorium, the diagnosis is usually only too obvious.

Out of the 93 cases treated at the Mitcham Sanatorium in the past two years, the principal cause of error in diagnosis was hydatid in two cases and associated with persistent hæmoptysis in both, and one case of small mediastinal tumour, discovered only at *post-mortem* examination, and causing severe paroxysmal cough, hæmoptysis, and dyspnoea.

DIET.

It is not now the usual practice to insist upon huge quantities of food being swallowed, and the mere gain in weight is not an index of the real progress of the patient. A standard diet has been fairly worked out, and usually gives a result of a gain of from 1 to 2 pounds per week, according to the sex and physique of the patient. Excess of weight has its penalties in causing dyspnoea, feeble circulation, dyspepsia, and easily induced fatigue. I usually advise patients to gain weight to the extent of 7 to 10 pounds over their best weight when they were in good health.

Bordswell and Chapman, *Diets in Tuberculosis*, 1908, page 165, give the following directions to home or out-patients:—

1. You require to take much more food than you did before you became consumptive.
2. Do not hurry over your meals, you will feel satisfied frequently before you have eaten enough, and you must continue to eat even when you do not want more food. Your appetite is no guide as to the amount you need. The best way to find out whether you are eating enough is to weigh yourself every week, always at the same time, and in the same clothes.
3. If you have not gained at least 1 pound during the week, you will know you have not eaten enough.

The poor consumptive is greatly in need of some instruction in the best and most economical foods to buy, and in cheap and rapid modes of cooking them.

In addition to the ordinary methods of treatment, I have administered tuberculin and other vaccines to 42 cases of the total number of cases, 93.

TURBAN'S CLASSIFICATION.

Stage 1. 16 cases.—Arrested, 14; much improved, 2.

Stage 2. 21 cases.—Arrested, 11; much improved, 9; not improved, 1.

Stage 3. 5 cases.—Improved, 3; not improved, 2.

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42

Cases treated without Tuberculin.

Stage 1. 17 cases.—Arrested, 16; dead, 1.

Stage 2. 8 cases.—Arrested, 4; improved, 2; not improved, 1; dead, 1.

Stage 3. 26 cases.—Arrested, 3; improved, 5; not improved or worse, 5;
— dead 13.

51

Although these results show very little one way or the other, I have inclined to think that the cases that have received tuberculin has improved more rapidly, and have kept their improvement better than the other class.

Standard normal .8 to 1.2.

Estimations of the opsonic index of the 42 cases treated with tuberculin —(T. R. Koch) doses varying from mg. . /5000 to 1/700 mg.—fifteen were controlled by the opsonic index (eliminating cases whose opsonic index was worked out less than four times.) The blood specimen was taken before administration of T.R. in the morning before exercise, and the control

specimen taken at about the same time. The indices were subsequently taken ten days after the injection of the tuberculin, when the patient is supposed to be in the positive phase. Consideration of expense to the patient precluded its being done oftener.

In every instance when the initial index was sub-normal, it was raised and maintained either within the normal limits or above. In one instance only did it fall, the initial index being 1.1 before T.R. was administered, and on the last examination had only fallen to .78.

In three advanced cases, I have administered, co-incidentally, vaccines prepared from the patient's own expectoration, other than the T.B., eliminating, as far as possible, contamination of the sputum by the organisms of the mouth, or those of food.

The patients certainly improved; but I cannot say that the disease is yet arrested. The opsonic indices rose after the injections, and were maintained at or above normal.

These opsonic estimations were made for me at the Bacteriological Laboratory of the Melbourne University, under the supervision of Dr. Bull.

The case of suspected tuberculosis, which ultimately proved to be hydatid, had her opsonic index taken on four occasions, and was remarkably steady. She had T.R. administered. They were 1.1 before T.R., .86, .8, .78.

Recently, we have heard of the apparently good effect of the administration of horse serum and tuberculin together given on an empty stomach. Corroborative of this, about eighteen months ago, I gave dried horse serum in two cases of obstinate hæmoptysis. in the hope of increasing the coagulative power of the blood. At the same time, I was giving both of the cases tuberculin injections. The hæmoptysis soon gave no trouble, and both cases, although they had made little apparent progress for several months, began to improve rapidly. Since then, I have used it regularly as a therapeutic agent. The serum was given by the stomach, and the tuberculin injected hypodermically.

SUMMARY OF RESULTS OF CASES TREATED AT MITCHAM SANATORIUM, 1ST JUNE, 1906, TO 1ST JUNE, 1908.

Arrested	...	...	...	...	43	
Improved slightly	...	...	...	...	3	
Much improved and improvement maintained	...	...	...	...	23	
					—	69
Not improved or worse	...	...	...	...	6	
Relapsed	...	...	...	...	2	
Died (one case only died at the Sanatorium, and that within three days of admission)	...	...	...	...	16	
					—	93
Hydatid disease, no tuberculosis	...	...	...	...	2	
					—	95

Hydatid disease, in my experience, has been the commonest cause of difficulty in diagnosis, two cases.

Although the question of treatment by tuberculin and other vaccines, with the aid of other specific products, is not strictly within the limits of this paper, my apology for putting these observations before you must be found in the fact that these problems in regard to phthisis can best be worked out in the sanatorium.

ON THE VALUE OF THE "OPHTHALMO REACTION" IN THE DIAGNOSIS OF TUBERCULOSIS.

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At a meeting such as this I need not waste time in emphasizing the paramount importance of early and accurate diagnosis in the modern treatment of tuberculosis in all its forms. We are agreed that success in treatment is in direct proportion to the stage of the disease when it comes under our care. Nor need I burden you with the already voluminous literature on the subject and the varying views of different observers.

This paper embodies the results of an investigation carried out to determine the value and limitations as a diagnostic agent of the "Ophthalmic reaction."

Three forms of tuberculin were used—(1) Calmette's original dried tuberculin, (2) a washed precipitate of ordinary old tuberculin prepared by myself, (3) Allenbury's Tuberculin Powder.

In all, 429 tests were made in 339 individuals. In 279 cases, a 1 per cent., in 60 a $\frac{1}{2}$ per cent., solution was used; 58 confirmatory tuberculin injections were given.

One hundred and seventy-seven cases were tested in hospital and private practice; 162 in the convalescent wards of one of the large metropolitan hospitals for the insane.

Method.—Dilution and instillation were made with an ordinary medicine dropper, using sterilised water as a solvent. No solution of more than 24-hours' age was used. It was found that, though nominally 1 cc. equals 17 minims, in practice the number of drops to a cc. varied with the dropper, so, in diluting, seventeen drops with the dropper held vertically was taken as 1 cc., and, in instilling the drop into the eye, care was taken to hold the dropper in the same position. In this way, a constant dose of 0.6 mg. tuberculin powder was assured. This dose, you will note, is ten times stronger than a 1 per cent. dilution of old tuberculin recommended by many German observers, and probably accounts for the more satisfactory results obtained.

Before instillation, the eyes were carefully examined and compared, the patient being directed to look upwards, to right, and left, and the conjunctiva of the lower lid was noted. Where one eye was paler than the other, that eye was used for the test. No eye showing active conjunctivitis was tested, though in several the conjunctivæ were red and injected at time of testing.

At the end of 8 and 24 hours, the eyes were again examined and compared. The time of onset of reaction varied from $2\frac{1}{2}$ to 18 hours. The reaction was always present, though sometimes much less marked at the end of 24 hours. No cases of reaction occurring after 24 hours were noted. The degree of reaction varied from very slight redness and swelling of the plica and caruncle to marked redness and swelling of the palpebral and ocular conjunctiva, with photophobia and purulent secretion. The number of severe reactions was small, and occurred, for the most part, in children. The majority of reactions were mild, and so much less striking than anticipated that they tended to be overlooked by nurses and residents until they had seen a number of cases.

The duration of the reaction varied with its severity. Slight reactions were gone in 48 hours, generally in three to four days. Rarely the

reaction lasted a week, and in one case, to be mentioned later, seven weeks. As a rule, the patient made little complaint of discomfort. Secondary reactions, as the result of injection of tuberculin, were often more severe than the original reaction.

RELATION BETWEEN DEGREE OF REACTION AND CLINICAL ACTIVITY. OF DISEASE.

Only a rough relationship can be traced under this head. Of 65 cases in which the degree of reaction is carefully noted—

Cases of slight apparent activity of disease gave 6 slight, 11 moderate, 3 marked reactions.

Cases of moderate apparent activity of disease gave 5 slight, 5 moderate, 9 marked reactions.

Cases of marked apparent activity of disease gave 4 slight, 14 moderate, 9 marked reactions.

Close correspondence between the degree of reaction and apparent activity of disease cannot be expected, as the extent of physical signs is no measure of the activity of the process, or of rate of liberation of toxins. In the case of joints where activity can be more accurately gauged, correspondence between the two is more marked, as it is, too, in lung cases which have improved and are nearly quiescent.

GENERAL RESULTS.

Of the 339 cases, 181 gave a positive, 150 a negative, result, while in eight the reaction was doubtful.

Where any doubt could exist as to the correctness of the reaction, whenever possible, a confirmatory tuberculin injection was given. The results of confirmatory tuberculin injection are open, unfortunately, to the criticism that the injections were in some cases too large owing to the dilutions having been made, on the assumption of Bosanquet, that old tuberculin contained 10 mg. tuberculin per cc. This gives doses 100 times as large as those made by diluting according to Koch's directions. On the other hand, there seems little positive ground for the statement that non-tubercular individuals may react to doses larger than 10 mg. The fact of an apparently healthy individual reacting is no proof that the reaction occurs in non-tubular subjects, and the fact that healthy guinea-pigs stand relatively enormous doses without reacting, while infected guinea-pigs are extremely susceptible to the test is in favour of a reaction meaning the presence of some focus, even when it is elicited by large doses in the apparently healthy.

That large doses may be given in healthy individuals without reaction is shown by the fact that—

In one case, 50 mg. was given without reaction; in another (hydatid of liver) 33 mg.; in one, 20; and in two, 33 mg.

Of seven cases negative to the eye test in which 10 mg. were given, in one only was the result positive, and in that case the doubt still remains whether it was tubercular or syphilitic.

A child negative to the eye test was negative to 33 mg.

A girl with cough of six-months' duration, loss of weight, and persistent evening temperature of 99-100, reacted to the eye test, gave no reaction to 20, slight to 40, definite to 75 mg.

That a dose of 10 mg. is not always enough to evoke a reaction in a definitely tubercular case is shown by a case of tubercular kidney with tubercle bacilli in the urine, which reacted to the eye test, was negative to 10 mg., but reacted to 20 mg.

More scientifically accurate information than at present obtainable is wanted on the question of what dose should be considered the limit non-reaction to which excludes the presence of tuberculosis, without entailing the risk of reaction in the non-tubercular. Some observers place this dose at 30 mg., while others consider 10, or even 5, mg. the limit.

Positive Cases.—Of the positive cases (Table I.), excluding the 97 asylum cases, in all of whom it was impossible to carry out confirmatory injection, there remain 84 positive cases. Of these, 73 from clinical signs, or from the results of confirmatory tuberculin injection, were undoubtedly tubercular. In two (one case of enteric and one of syphilis), the tuberculin test fully, in two (one case of enteric and one of syphilis), partially, confirmed the eye test. In seven control cases the patients left hospital before confirmatory injection could be given.

Excluding the asylum cases and those in which confirmatory injection could not be given, also the enterics and syphilitics, which will be dealt with later, we have left 73 positive results, in all of which the reaction was undoubtedly correct. Nine of these were simply suspected cases, and three controls, the correctness of the reaction being proved by tuberculin injection.

Negative Cases (Table 2).—Excluding the asylum cases, we find that of 91 negative cases, 76 clinically non-tubercular gave no reaction; six cases of healed tuberculosis gave no reaction; one case of enteric, one of syphilis, and one of swelled testicles, gave a negative eye reaction, but, after a second instillation, reacted subsequently to 10 mg. tuberculin injection; one case of probable tubercular meningitis failed to react, as did one with basal signs, which subsequently reacted to 20 mg. tuberculin, and one case in which tubercle bacilli were found in sputum two months later. One negative case failed to 10 mg., but reacted to the enormous dose of 50 mg.; one reacted feebly, and one doubtfully, to the same dose (50 mg.).

Excluding enteric and syphilis, we have 72 negative cases, in one of which tuberculosis was certainly, in two probably, in one doubtfully, present, giving a maximum error of 6 per cent.

No moribund cases were tested.

SECONDARY SYPHILIS.

Arloing states that in secondary syphilis in full activity a positive reaction occurs, which however, always appears late. In this series, twelve cases were tested. Three gave a positive reaction. Of these, one reacted well to 20 mg. tuberculin injection, one feebly to 33 mg., one was not tested. One case negative to two instillations reacted to 33 mg.

It appears probable that the reaction may occur in secondary syphilis, as has long been known to be the case with subcutaneous injection.

CONVALESCENT ENTERICS.

Levy states that convalescent enterics may give the Ophthalmic reaction, and Cohn got 26 positive results in 54 cases of typhoid, one of which died and showed no tubercular lesions at the autopsy.

In the above series of twelve tested, only two gave a positive reaction. Of the positive cases, one reacted definitely to 33 mg., having failed to 10 mg. tuberculin injection, one doubtfully to 20 mg.

Of the negative cases, one after a second instillation, reacted later to 33 mg. tuberculin, though showing no sign of tuberculosis.

As far as may be judged from so small a number of cases, it would appear that a small percentage of convalescent enterics, apparently free from tuberculosis, react to the eye test, and also to subcutaneous injection where the dose is large.

RESULT OF SECOND INSTILLATION OF TUBERCULIN INTO THE SAME EYE.

This is an important point, want of knowledge on which has led to much of the discordance between the results of different observers.

Levy, for instance, warns against second instillation, stating that it gave positive results in three-fourths of his non-tubercular cases. He also states that a third instillation may give rise to very serious re-action. Other observers use two, or even three, instillations at varying intervals.

At first, from a small number of observations, I concluded that where the first instillation had been negative, it could be safely repeated, and the result trusted, if done within eight days. Further experience, comprising 67 cases, has shown this to be incorrect.

From the attached table (Table III.), it will be seen that none of the cases repeated the next day gave a positive result, while 43 per cent. of those done after three days, 61 per cent. done after five, 50 per cent. after seven, 75 per cent. done after nine, and all, except one, done at a longer interval, up to 107 days, gave a positive re-action.

These results have an important practical bearing on the applicability of the test. They show that if the first test made with a weak solution is negative, and doubt exists as to its accuracy, a second test, to be free from source of error, must be done next day. A positive result on and after the third day is of no value in determining the presence of tuberculosis.

They also throw much doubt on the results of those who instil a weak solution into one eye, then a stronger into the other, and, again, a stronger into the eye first tested.

The fact that a reaction is obtained on a second instillation in an eye that has not reacted to the first test, as long as 107 days after the first instillation, has important bearings.

In the first place, it makes it incumbent on the physician to explain clearly to the patient what has been done, otherwise an installation into the same eye by another practitioner in ignorance of the first test would give rise to grave error. Again, these results show that the condition of anaphylaxis, or increased sensitiveness to tuberculin produced by the first instillation, lasts some time. The same condition of things is produced by the disease itself, so the fact of a negative result in an apparently healed case means, not only that arrest has taken place, but also that sufficient time has elapsed for the tissues to have lost their anaphylactic condition, or that immunity to the reaction has been established. How long that is, is yet undetermined, but the cases quoted above show that in some cases it is, at least, 107 days.

RESULTS OF SECOND INSTILLATION INTO OPPOSITE EYE IN
NEGATIVE CASES.

Table V.

Cases.		Interval. Days.		Result.
1	...	2	...	negative
5	...	5	...	"
4	...	7	...	"
6	...	7	...	positive
1	...	9	...	? in 8, negative in 24 hours
1	...	5 & 14	...	negative
2	...	12	...	"
1	...	14	...	"
1	...	12 & 21	...	"
1	...	34	...	"

The results in these cases are interesting, but the number of experiments is too small to allow of absolute deductions. It will be noted that of 23 tests done at periods varying from 1 to 34 days, all were negative except when done on the seventh day. Here, of ten cases, six gave positive results.

In view of the fallacious results occurring in my original small series of positive cases, I should not feel justified in stating that the result of repetition in the opposite eye was reliable without a much larger series of experiments from which to draw conclusions.

RECRUDESCENCE OF EYE REACTION ON INJECTING TUBERCULIN.

Early in this research we were surprised to find that the injection of a confirmatory dose of tuberculin was generally followed by recurrence of the eye symptoms in eyes that had already reacted, and its appearance in eyes which had not responded to the original test. In some cases, this second reaction was much more severe than the original reaction, going on, as noted above, in one case to phlyctenular ulceration.

In three cases in which only one eye was tested, both eyes reacted on injecting tuberculin, the reaction in the untested eye being very mild. These cases may possibly be explained on the hypothesis that some of the fluid was accidentally transferred from one eye to the other by the patient at the time of instillation.

The attached tables show the relationship between the interval between the two tests and the occurrence or not of eye symptoms (1) in cases where the eye had not originally reacted, (2) where the eye had reacted, and (3) where the tuberculin injection was negative.

Summarizing the results—Of fourteen cases where the injection follows within five days of instillation, only in three was there recurrence of eye symptoms; of four done on the fifth day, two reacted; while in all the cases, except one, 40 in number, done after the fifth day recurrence of eye symptoms occurred, the reaction being most severe in those done up to the fifteenth day.

From these results, we gather that where a tuberculin injection is done to control the ophthalmic tests, such injection should be given either before the fifth day or after about three weeks, to avoid setting up a second eye reaction.

EXCESSIVE REACTION.

In three cases in this series the eye reaction was severe, in two excessively so.

1. A case of secondary syphilis, whose eyes before testing were considerably injected, reacted severely with photophobia, bulbar injection, chemosis and purulent discharge. The signs gradually subsided in the course of a week. An injection of tuberculin given on the eighth day caused a severe recrudescence lasting a week.
2. A case of debility and slight fever, without signs, re-acted sharply to 1 per cent. solution. A confirmatory injection given six days later caused violent reaction, with chemosis and photophobia lasting a week.
3. A girl of sixteen years with a discharging tubercular knee reacted sharply. A fortnight later, injection of 10 mg. tuberculin was followed next day by reaction in both eyes. The eye originally tested became very injected, the cornea hazy, and a small phlyctenular ulcer formed at the margin of the cornea. The eye cleared completely in the course of a fortnight. In this case, a history was subsequently obtained of previous trouble in that eye.

In these three cases, the original reaction, though rather severe, owed its main severity to tuberculin injection.

4. A girl of 25, with slight signs at one apex and a temperature of 100, was tested on 5th May, 1908, with 1 per cent. A. & H. tuberculin. A sharp reaction occurred, and, against advice, she left for the country next day. On 23rd June, 1908, she wrote that the eye was still very bad, and causing severe headache. Her medical attendant, Dr. Stanley, with whom I at once communicated, wrote that on 16th June, 1908, when first seen—"There was conjunctival congestion (slight), slight photophobia, and three small corneal ulcers. In about four or five days, with an eye lotion of cocaine, borax, and atropine, it was well."
5. A girl of 21, with slight signs and glands in neck, probably due to pediculosis, received a $\frac{1}{2}$ per cent. solution of A. & H. on 24th June, 1908, the reaction was marked. When seen again on 6th August, 1908, *i.e.*, six weeks later, the eye was still red and injected. She said it was better then, but had been very sore. "Got better and worse, and watered a lot."

As the result of careful search through the literature, I have found records of thirteen cases of excessive reaction, making, with my own, fifteen cases. At the same time, I have found recorded 3,223 tests, excluding several series where the number of tests is not definitely stated. In five cases of excessive reaction there was either active trouble in the opposite eye, or history of previous trouble in the tested eye. In two, the test was repeated in the same eye three days after failure with a weaker solution. Here, as shown above, the eye was probably sensitized by the first test. In six of the excessive cases, the patients were children, and a 1 per cent. solution was used.

Excluding the cases where the eyes were originally unhealthy, or where the test was repeated on the third day, we have eight excessive results in at least 3,223 cases, a proportion of 1 in 400, or, including all cases, 1 in 215.

CONCLUSIONS.

1. Where a $\frac{1}{2}$ oz. or 1 per cent. solution of Calmette's Tuberculin Powder is used, a positive re-action appearing within, and present at the end of 24 hours is, in the absence of convalescent typhoid or secondary syphilis, very strong evidence of the presence of an active or recently quiescent tubercular focus somewhere in the body.

2. A negative re-action, though not absolutely excluding, is strongly against the presence of tuberculosis.

3. Absence of re-action in clinically arrested cases is of the same value as negative result of tuberculin injection, as a proof of actual arrest.

4. Where, in the case of failure of the first test, it is repeated in the same eye, to be of value the test must be made the next day. After that time a positive result is valueless.

5. The patient should always be clearly informed of what has been done when the test is made, as a positive result is of no value unless it is certain that the eye has not been previously tested.

6. Where confirmatory tuberculin injection is performed, it should be done within five days, or at the end of three weeks, to obviate secondary eye re-action.

7. The eye test may interfere with subsequent treatment by injection, unless the treatment be postponed for a few weeks after doing the test.

8. As there is some, though slight danger of setting up severe inflammation in the eye, the test should not be performed as a routine to save trouble, but should be reserved for those cases in which the ordinary methods of diagnosis fail, should be used only by those who have made themselves conversant with the sources of error in its use, and the patient should be clearly informed of the slight risk of eye trouble possibly following. It should never be used where there is active trouble in either eye or history of trouble in the tested eye. In children, never more than $\frac{1}{2}$ per cent. solution should be used, probably $\frac{1}{4}$ per cent. is strong enough.

9. Used with these precautions, the ophthalmic re-action is a very valuable addition to our means of detecting tuberculosis in an early stage.

TABLE I.

The 181 positive cases were distributed as follows:—

- 15 early cases of pulmonary tuberculosis.
- 17 moderately advanced or advanced cases of pulmonary tuberculosis.
- 4 pleurisy (one operated on in error as duodenal ulcer) confirmed by tuberculin injection.
- 1 pleurisy, recurring, and probable spinal caries, confirmed by tuberculin injection.
- 4 cases suspected from persistent temperature, loss of weight, debility, or other cause, confirmed by tuberculin injection.
- 2 slowly resolving pneumonia, one confirmed by tuberculin injection.
- 1 chronic foetid bronchorrhœa, pleurisy, fever, and hæmoptysis.
- 1 diarrhœa and persistent crepitations at right base.

- 1 pulmonary tuberculosis and tubercular knee.
- 1 hæmoptysis and fever.
- 1 hæmatemesis, confirmed by tuberculin injection, 20 mg.
- 9 tubercular joints.
- 1 tubercular astragalus.
- 2 doubtful knees, confirmed by tuberculin injection, 20 mg.
- 1 psoas abscess.
- 1 caries of spine (negative to 10 mg. tuberculin).
- 3 tubercular glands.
- 1 tubercular glands and syphilis, confirmed by tuberculin injection, 20 mg.
- 1 tubercular glands and gonorrhœa, confirmed by tuberculin injection, 20 mg.
- 1 tubercular kidney (T.B. in urine).
- 1 irregular temperature and mitral murmur, thought at first to be endocarditis, confirmed by tuberculin injection.
- 1 cerebral embolism with valvular disease, confirmed by tuberculin injection, 20 mg.
- 1 aortic regurgitation, with pleural effusion, and locomotor ataxia, showed P.M. scarring and adhesions at right apex.
- 1 case which a year before had had hydropneumothorax, had been under open air treatment, had lost all signs, but at time of test had temperature 99.
- 1 case slight signs one apex, and chronic ulceration of eyelids.

In these 73 cases, there is no doubt of the correctness of the diagnosis.

Of the remainder :—

- 2 were convalescent enterics. One re-acted to 33 mg. of tuberculin, after failing with 10 mg. The other, whose original eye re-action was slight, in 36 hours re-acted to 99 with 20 mg., when through some mistake, no further observation was made.
- 2 were cases of secondary syphilis. One re-acted to an injection of 20 mg. In the other, 36 hours after injection of 33 mg., the temperature rose to 99.6.
- 1 case of gonorrhœa and syphilis went out before controlling injection could be made.
- 1 control, rheumatism went out unchecked by injection.
- 1 control, heart disease went out unchecked by injection.
- 1 control, exophthalmic goitre went out unchecked by injection.
- 1 control, hemiplegia went out unchecked by injection.
- 1 control, amputation with scars of old glands in neck.
- 1 "neurasthenia."
- 97 were cases done at one of the Hospitals for Insane. It being impossible to control all by injecting with tuberculin, two of the most unlikely to be tubercular were injected, and re-acted well.

TABLE II.

Of the 150 negative cases—

- 1 dyspepsia, no suggestion of tuberculosis.
- 1 convalescent from operation for fistula, no sign of tubercle.

- 1 neuritis left leg.
- 1 malignant disease of bladder.
- 1 contracture from burns.
- 1 early locomotor ataxia.
- 1 lymphatic obstruction of leg.
- 1 chyluria.
- 2 chronic valvular disease of heart.
- 1 rheumatic endocarditis.
- 1 acute, 2 subacute, 5 chronic nephritis.
- 1 subacute rheumatoid arthritis in adult.
- 1 subacute rheumatoid arthritis in child, confirmed by tuberculin, 10 mg.
- 1 chronic rheumatoid arthritis, confirmed by tuberculin injection.
- 9 convalescent enterics.
- 7 secondary syphilis, 1 gonorrhœa and syphilis, 3 gonorrhœa.
- 1 swelled testicles, confirmed by tuberculin injection.
- 1 pleural effusion, negative to 10 mg. tuberculin.
- 1 acute, 1 convalescent pneumonia.
- 1 diabetes insipidus, 1 chlorosis, 1 pernicious anæmia.
- 1 simple anæmia, 1 gout and plumbism, 1 chronic malaria, 1 hemiplegia, 1 aneurism, 1 beri beri, 1 catarrhal jaundice, 1 G. P. I.
- 2 appendicectomy, 1 urethral stricture, 1 hysteria, 2 influenza.
- 1 pyuria (T. B. absent), 1 lumbar abscess, 1 papilloma of larynx.
- 2 rheumatic fever, 1 cerebrospinal meningitis, 1 synovitis of knee, negative to tuberculin injection, 12 mg.
- 1 empyæma and secondary cerebral abscess.
- 1 hip disease, quiescent, on back for eight months, no pain, perfect movement.
- 1 slight rise of temperature, no sign, left hospital in few days well, but returned two months later with irregular fever, slight signs in lungs, and tubercle bacilli in sputum.
- 1 supposed chronic pulmonary tuberculosis, under observation for five years without change.
- 1 "spat blood," negative to 10 and 33 mg. tuberculin.
- 1 pain in back, probably growth.
- 1 ruptured hydatid, negative to 33 mg.
- 1 harsh breathing at apices of lungs, negative to 50 mg. tuberculin.
- 1 ankylosed hip, probably tubercular, up and about for five months after seven months' rest.
- 1 caries of spine detected in early stage one year before, on back nine months, about and well three months.
- 1 early pulmonary tuberculosis, arrested two years ago; at onset, T. B. found in sputum.
- 1 case of summer diarrhœa, signs of old consolidation at right apex, no symptoms known to patient.
- 1 case of swelling of side of knee for five months. This case reacted to 99.8 with 25 mg. tuberculin; but no recurrence of eye reaction. Proved on operation to be myloid sarcoma.

- 1 doubtful ankle with sinuses. Reacted fourteen days later to 100 with 50 mg. tuberculin; no four-hourly chart had been kept previous to injection, temperature had been irregularly raised before and remained up for a week. Reaction cannot be relied on.
- 1 swelling of both testicles, without involvement of vas, vesiculæ, or bladder, which cleared up considerably under rest and large doses of pot. iod., in a man of 21. There was no syphilitic history, except that hair fell out in patches. In this case, the first test was negative, a second 33 days later into the same eye was positive, as was expected; fourteen days later he reacted to 100 in 32 hours; and later to 101 with 10 mg., without signs of focal reaction, but with local skin reaction and recrudescence of eye symptoms. This case was quite possibly tubercular, though one would have expected some tenderness of testicles on injection.
- 1 case of acute enteric with pneumonia, and positive Widal gave in acute stage negative reaction; 22 days later a second test in the same eye gave, as expected, a positive reaction. Fourteen days later 33 mg. injection of tuberculin gave a reaction to 103.6, with severe recurrence of eye symptoms.
- 1 case of meningitis, clinically tubercular, in which test was made two days before death.
- 1 case of rapid pulse, persistent crepitations at both bases, slight fever, sputum repeatedly negative for tubercle. gave negative eye reaction, but six days later reacted to 102 with 20 mg. tuberculin.
- 1 case cough and run down for three weeks, no temperature or chest signs. Some lachrymation at time of instillation. Reacted fourteen days later to 99.4 with 50 mg., with considerable constitutional disturbance and local skin reaction. The eye now reacted.
- 1 case of secondary syphilis 28 days after two negative instillations into the one eye, reacted to 33 mg. tuberculin.
- 1 case (115) flattening at right apex with harsh breathing and persistent temperature of 100 degrees failed to 10 mg., reacted to 50 mg. ten days later; three days later gave negative eye test; 28 days later failed to 10 mg., 7 days later to 20 mg.; 95 days after first eye test repeated in same eye with slight positive result; 16 days later negative to 33; and 2 days later to 100; eye did not now react.

59 were apparently healthy lunatics.

Doubtful cases—

- 1 case of neurasthenia, negative to 20 mg. tuberculin.
- 1 case of gastric ulcer not checked by tuberculin.
- 6 were lunatics, in whom the reaction was so indefinite as to be doubtful.

TABLE III.

Result of second instillation into same eye in negative cases, combining results of $\frac{1}{2}$ per cent. and 1 per cent. instillations.

Cases.	Interval in Days.		Result.
4	...	1	negative
2	...	3	negative
4	...	3	positive
1	...	3	(?)
8	...	5	negative
14	...	5	positive
1	...	5	(?)
7	...	7	negative
7	...	7	positive
1	...	9	negative
3	...	9	positive
1	...	21	positive
1	...	22	positive
2	...	33	positive
1	...	95	positive (?)
7	...	100	positive
2	...	107	positive
1	...	107	negative

That is, none of the cases repeated the next day gave a positive result, while 43 per cent. of those done after 3 days, 61 per cent. done after 5, 50 per cent. done after 7, 75 per cent. done after 9, and all except one done at a longer interval up to 107 days gave a positive reaction.

TABLE IV.

Result of second instillation into same eye in negative cases.

Using 1 per cent. solution.

Cases.	Interval in Days.		Result.
3	...	1	negative
7	...	5	negative
9	...	5	positive
7	...	7	negative
7	...	7	positive
1	...	9	negative
3	...	9	positive
1	...	21	positive
1	...	22	positive
2	...	33	positive

Using $\frac{1}{2}$ per cent. solution.

Cases.	Interval in Days.		Result.
I	...	1	negative
2	...	3	negative
4	...	3	positive
I	...	3	(?)
I	...	5	negative
5	...	5	positive
I	...	5	(?)
I	...	95	positive (in this case possibility of active tuberculosis being present) though negative to large doses of tuberculin.

TABLE VI.

Recrudescence of eye action on injecting tuberculin in eyes that had already reacted to the ophthalmic test.

Cases.	Interval in days between eye test and injection.		Result.
1	...	1	reaction in eye much increased.
I	...	2	negative
4	...	3	negative
2	...	5	positive
3	...	4	negative
2	...	4	positive
I	...	6	"
I	...	7	"
4	...	8	"
I	...	9	"
1	...	11	"
2	...	12	"
3	...	14	"
I	...	14	both eyes reacted, and an ulcer appeared in the tested eye
I	...	17	both eyes reacted; but did not react again on repeating injection on 2nd and 4th day after.
3	...	19	positive
2	...	29	"
I	...	37 & 51	"

That is, of 11 cases tested before the fifth day, 3 reacted; while of 23 tested on or after the fifth day, all reacted.

TABLE VII.

Recrudescence of eye reaction on injecting tuberculin in eyes that had not reacted to the ophthalmic test.

Cases.	Interval in Days.		Result.
I	...	I	negative
I	...	5	..
I	...	6	positive (both eyes)
2	...	14	negative
I	...	14	positive
I	...	6, 8, 15	negative (10, 20, 33 mg. given to small girl)

TABLE VIII.

Recrudescence where eye, negative to first, reacted to second instillation.

Cases.	Interval in Days.		Result.
3	...	5 after second and 14 after first	positive
I	...	8 after second and 35 after first	..
I	...	14 after second and 36 after first	..
I	...	14 after second and 47 after first	..

Where eye had not reacted to first or second instillation.

2	...	17 after second and 24 after first	positive
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TABLE IX.

Recrudescence of eye reaction on injecting tuberculin in cases where the tuberculin test was negative.

Cases.	Interval in Days.		Result.
2	...	3	negative
I	...	5	..
2	...	5 & 14	positive
I	...	17	..
I	...	17 & 24	..
I	...	8, 33, & 35	..
I	...	6, 8, & 15	negative

COMPLICATIONS FOLLOWING OPHTHALMO REACTION.

Knap, Arnold (*Archives of Ophthalmology*, March, 1908) relates case where several typically tubercular corneal infiltrations occurred ten days after instillation of 1 per cent. solution in girl of nine years, whose other eye was suffering from active superficial keratitis of two months' duration.

Kalt, at Paris Ophthalmological Association, on 7th October and 5th November, 1907, spoke of patient with quiescent sclerokeratitis, in whom two days after 1 per cent. instillation, condition became aggravated with iritis and increasing corneal involvement, which lasted one month, and sight was reduced.

Cates, J. (*B.M.J.*, 25-4-08).—Case of cervical glands and history of watering of right eye. At time of test eye seemed normal. Severe reaction settling down in six days. On seventeenth day, patch of keratitis on lower edge of cornea going on to ulceration.

Case of cervical glands tested from same tube on same day: severe reaction and recurrent inflammation of conjunctiva caruncle followed.

Ramsey, A. M. (*Lancet*, 7-3-08).—Girl, 12, superficial vascular ulceration right eye, cervical and submaxillary glands enlarged, no other sign of tubercle. History of similar attack in left eye two years before. 10th October, 1 per cent. solution into left eye, on which faint nebula from old ulceration. Violent reaction; cornea became vascular and abrasion in centre. Treated with tuberculin, gradually improved, but left considerable opacity in centre of cornea.

Barbier, H. (*Bulletins des Hospitals*, 6-12-1908, *Med. Rev.*, May, 1908).—Boy 13, nephritis, anasarca, double pleural effusion, congestion right apex. At beginning of 1906 had suffered keratitis for a month: 10th August, 1 per cent. into right eye, intense reaction, next day superficial ulceration of cornea. At beginning of September, left eye became affected, and at end of month left cornea completely clouded with ulcers and pannus, and ocular conjunctiva red and œdematous. Same lesions to less degree in left eye. Did not begin to subside till end of October. At time of report, only two little opacities, one near centre of each eye, which did not interfere with vision. Thus acute ulceration for three months.

Renon, Louis (*Ibid.*, p. 1396).—Of 28 cases tested with 1 per cent., severe results in three. In one conjunctivitis lasted 45 days, in second slight keratitis occurred, in third interstitial keratitis and extensive iritis attended with adhesions, which had to be broken down with atropine and eserine.

Mackay, M. (*Boston Medical and Surgical Journal*, 12-3-08, *Med. Rev.*, July, 08).—Woman with tubercular knee, spine and lung. Drop 1 per cent. on 14-12-07 gave marked reaction subsiding in ten days. Recrudescence on 20th January, becoming severe, with chemosis. On 26th February, more than ten weeks after instillation, conjunctiva red, with slight photophobia, and slight narrowing of the palpebral fissure.

Butler, Harrison (*B.M.J.*, 8-8-08).—Child, 4, ankle regarded as tubercular, eyes normal. 6-3-08, $\frac{1}{2}$ per cent. in right eye, negative. On 9th (*i.e.*, three days later) 1 per cent. in same eye. Next day decided reaction, which got steadily worse. Chemosis, swelling of eyelid, thickening of eyelid, gradually yielded to treatment, but photophobia and lachrymation persisted. Early in April, elongated phlyctenule at margin of cornea. Got gradually worse, and at end of April three distinct patches of interstitial keratitis, and an ulcer size of pin's head in centre of cornea. Gradually cleared up, leaving a small central nebula.

Case 2. Girl, 7, tubercular knee, one drop 1 per cent. into right eye on 3-9-08, violent reaction, developed into chronic conjunctivitis. Early in April, phlyctenule, which persisted till early in May, and still some lachrymation and photophobia.

Case 3. Little boy, tubercular ankle, $\frac{1}{2}$ per cent. negative, repeated 1 per cent., violent conjunctivitis lasting two months. Eye previously healthy.

THE AMBULATORY DIAGNOSIS AND TREATMENT OF PULMONARY TUBERCULOSIS.

J. KELMAR, M.D.

A perusal of the articles published in the various Australian medical journals during the last two years shows clearly that the *Ambulatory* treatment of pulmonary tuberculosis does not receive the attention of the medical profession in Australia to the same extent as in Europe. The sanatorium treatment appears to be the only recognised method of dealing with this disease. Observations extending over a number of years have convinced me that, by means of sanatoria alone, we will not be able to succeed in our crusade against tuberculosis. Those best able to judge are not so enthusiastic about its efficiency as they were some five or ten years ago. The percentage of consumptives absolutely and lastingly cured by the sanatorium treatment is only a small one, whilst relapses are not infrequent, and leading specialists in Europe have lately pointed out that the expenses of the sanatorium treatment are altogether out of proportion to the actual results achieved.

I believe I have been one of the first to advocate the *home* supervision and treatment of poor consumptives and their families, but I stand not alone in this respect. Professor Koch himself, in a Nobel lecture delivered at Stockholm in June, 1906, recommended, amongst other measures—"The treatment and supervision of poor consumptives in their homes if we cannot receive them into sanatoria." Then, again, a sub-committee appointed by the Committee of the Hospital for Consumption and Diseases of the Chest, at Brompton, in England, "to elaborate a system to deal more effectually with tuberculosis," have recommended "that they must try to get at the people who are ailing in health, and really suffering from tuberculosis, but are totally ignorant of this fact. A medical examination of the members of the family of a consumptive patient, their home supervision, and home treatment, are therefore necessary, otherwise it will not be possible to prevent the dissemination of the germs of the disease."

Considering our great climatic advantages, the sparseness of our population, and the fair economic conditions of our people, it is surprising that the decrease in the number of deaths from tubercular diseases has not been greater during the last decade. One of the causes is undoubtedly our frequent failure to detect the incipient cases of phthisis. The sanatoria, the Health authorities and the specialists, all say: Give us the early cases and we will cure them, but do not send us advanced consumptives. But there is another even more potent reason why our finer methods of diagnosis should be generally known and employed to enable us to get hold of the incipient cases. If we bear in mind that when a tubercular affection clearly manifests itself the infection has probably been present in the organism for a long time, perhaps for years, or even since childhood, at times latent, at others more or less active, although not recognisable by our usual methods of examination, it is apparent that such cases are a constant menace and one of the chief sources of spreading the infection.

To detect early cases of tubercular lung trouble the general practitioner, in his capacity as family physician, has undoubtedly the best opportunity, and we must therefore enlist his aid in order to subdue and control the disease. Unfortunately, at present, he still appears to rely upon the physical methods of examination alone for diagnosing pulmonary diseases, although it is generally known that these otherwise invaluable methods often fail entirely in incipient phthisis, and upon cough mixtures, cod-liver oil, *et hoc genus omne*, for treatment. Shall his co-operation be of

real help to us, he must understand and practise the specific diagnosis and the rational ambulatory treatment of tuberculosis. As regards the former, we have four different methods of causing a specific reaction in tubercular subjects—the probatory tuberculin injection, von Pirquet's cutaneous vaccination test, Wolff-Eisner's ophthalmic—or better conjunctival reaction and Moro's inunction method.

For the probatory tuberculin injection, I use—

1. A tuberculin solution consisting of Koch's tuberculin (old), 1 ccm., and a 0.5 per cent, sterile carbolic-acid solution, ad 100 grammes.
2. A bottle of a 0.5 per cent. sterile carbolic-acid solution.
3. B. W. & Co.'s all-glass, 1 ccm., hypodermic syringe, divided into twenty equal parts, with platino-iridium needles.

I commence with 0.1 mgr. tuberculin, increasing the dosage to 0.4 mgr., 1 mgr., 4 mgr., and 10 mgr., respectively, preparing the different solutions in the sterile syringe itself.

To obtain 0.1 mgr. tuberculin, one draws up of the tuberculin solution two divisions of the syringe, and of the 0.5 carbolic-acid solution, eighteen divisions, making 1 ccm., or the full contents of the syringe, shake well, and injects two divisions, or 0.1 ccm.

The injections are made hypodermically in the back, between or below the shoulder-blades; the part has to be disinfected with ether, and after the injection should be covered with some aseptic wool and a piece of adhesive plaster. During the three days preceding the probatory tuberculin injection, the patient should stay in bed, and his temperature be taken every two hours from 6 a.m. until midnight; a thorough examination of the lungs, the larynx, and the abdominal organs should also be made and the urine analyzed. After the injection, the temperature has again to be taken every two hours. A rise of temperature of $\frac{1}{2}$ degree C. is considered positive, and the same dose may be repeated after the temperature has again become normal; after the second injection, the reaction is, as a rule, more distinct.

Has there been no rise in the temperature, or one of less than $\frac{1}{2}$ degree C., 0.4 mgr. are injected two days after the first test; should there be again no reaction, or only a very slight one, 1 mgr. is injected in the same way, and so on, 4 mgr., and 10 mgr., if necessary. A negative reaction after the injection of two separate doses of 10 mgr. each is considered a sure proof of the non-presence of any tubercular lesion.

The maximum of the reaction is to be expected in from nine to twelve hours after the injection; it is general as well as local, but always of a transitory nature and mild, provided the above directions are carried out. Abdominal pains appear comparatively often after these injections, further, cephalalgia, spinalgia, palpitation of the heart, and malaise. It is undoubtedly a very reliable test, but as the whole procedure is rather complicated and the patient has to stay in bed for several days, it cannot be considered so suitable in ambulatory practice as the other three methods of specific diagnosis. In case of fever, even if only very slight, a probatory tuberculin injection should not be made.

The technique of von Pirquet's cutaneous tuberculin test is extremely simple. Required is a solution of Koch's tuberculin (old) and 5 per cent. carboglycerin, 1 gm. each, and 2 gm. of a sterile physiological saline solution (0.8 per cent.). After disinfecting the forearm with ether, two drops of this solution are allowed to fall upon the arm at a distance of

about 10 cm. from each other. With an ordinary vaccination lancet the epidermis is slightly scarified within each drop, and also between them, the latter for controlling purposes. After a few minutes the part is covered with aseptic wool. Should a positive reaction result, there appears within the next 24 hours a hyperæmic papule, having a diameter of from 5 to 15 mm., surrounded by a light red zone of the size of a half-a-crown piece; in some cases, this zone is not noticeable. The papule disappears again within from five to ten days.

The technique of Wolff-Eisner's conjunctival reaction is still simpler. For this test I use either Calmette's $\frac{1}{2}$ per cent. tuberculin test or a 1 per cent. solution of Koch's tuberculin (old), by Ruete-Enoch, of Hamburg, in sterile saline solution (0.8 per cent.). One or two drops of either are instilled into the nasal corner of one eye, while the eyelids are kept separated for about half a minute. If positive, the reaction sets in within from 6 to 24 hours. We can discern three degrees—

1. A reddening of the conjunctiva palpebræ inferioris et carunculæ lachrymalis;
2. The same as 1, with the participation of the conjunctiva bulbi;
3. Conjunctivitis purulenta.

The last degree is not desirable. A slight conjunctivitis of one eye is no contra-indication to applying the test to the healthy eye, but in tubercular or other diseases of the eye it should not be used. Personally, I have never noticed any ill-effects from this test.

Moro, of Munich, uses an ointment consisting of 5 ccm. of Koch's tuberculin (old) and 5 gm. Lanolin anhydric. A very small quantity, about sufficient to make a large pill, is, by gentle massage, rubbed into the skin of the epigastrium for about a minute; the part treated should be about 5 cm. in diameter. After allowing the ointment to become well absorbed, no further dressing is necessary. The reaction is positive if papular efflorescences appear. There are also three degrees of reaction—

1. A mild one if within from 24 to 48 hours after application a very few pale or light-red papules appear, having a diameter of from 1 to 2 mm., which disappear again after a few days.
2. A more pronounced reaction, showing within 24 hours numerous hyperæmic papules of different sizes up to a diameter of 3 mm., the surrounding skin is slightly inflamed, and there is also some irritation present. The efflorescences remain unaltered for a few days and disappear gradually.
3. A severe reaction—within a few hours after the inunction a large number of larger hyperæmic papules appears, and the skin surrounding each papule is strongly inflamed. There is also considerable irritation. Some of the papules are as large as 8 mm. in diameter, and contain a watery fluid. This dermatitis has the tendency to spread beyond the region treated. After a few days the efflorescences begin to dry up and scale, and in about two weeks nothing but a discolouration of the skin is noticeable.

How can we now explain this reaction? We know that under the influence of an infection specific anti-bodies are formed in the blood of the living organism. If now, for instance, this formation of anti-bodies is caused by a tuberculous infection, biological poisons are generated when these anti-bodies come in contact and combine with the tubercle bacilli or their extracts—tuberculin—causing a local inflammatory reaction. Should, therefore, the living cell not any more be able to form such anti-bodies as in advanced tuberculosis, no specific reaction can take place.

Of these three cutaneous tuberculin tests, Moro's inunction method is the most harmless, it is also easy to apply, and, therefore, eminently suitable for ambulatory practice. It has the additional, not inconsiderable, advantage that every practitioner can prepare the ointment himself. If kept in a cool dark place, it retains its activity for months. The lanolin should, previously to mixing it with the tuberculin, be warmed to from 20 to 30 degrees C. I give the results of the Conjunctival, von Pirquet's Vaccination, and Moro's Inunction tests in a few cases:—

		Number of Cases.	Results of Wolff-Eisner's Conjunctival Tuberculin Test.		Number of Cases.	Results of			
			Reaction.			Von Pirquet's Cutaneous Vaccination Test.		Moro's Percutaneous Inunction Test	
						Reaction.		Reaction.	
			Positive.	Negative.		Positive.	Negative.	Positive.	Negative.
Tuberculous patients		7	6=85·7 per cent.	1=14·3 per cent.	18	15=83·3 per cent.	3=16·7 per cent.	16=88·8 per cent.	2=11·2 per cent.
Scrofulous patients	pa-	3	3=100 per cent.	0	6	6=100 per cent.	0	6=100 per cent.	0
Suspects	...	5	3=60 per cent.	2=40 per cent.	6	5=83·3 per cent.	1=16·7 per cent.	4=66·6 per cent.	2=33·4 per cent.
Persons apparently free from Tuberculosis	ap-	30	6=20 per cent.	24=80 per cent.	15	3=20 per cent.	12=80 per cent.	4=26·6 per cent.	11=73·4 per cent.

RESULT OF THE CUTANEOUS TUBERCULIN TEST AFTER VON PIRQUET'S, WOLFF-EISNER'S AND MORO'S METHOD.

Authors.		Tuberculous Patients.			Suspects.		
		Reaction.			Reaction.		
		Number of Cases.	Positive.	Negative	Number of Cases.	Positive.	Negative.
Mainini ...	...	35	30	5	106	93	13
Schroeder and Kaufmann	...	48	43	5	9	4	5
Various Authors quoted by Schroeder-Kaufmann	by	971	859	112	284	146	138
Blum ...	...	26	21	5	5	3	2
Emmerich ...	...	46	28	18	51	44	7
Goebel ...	...	71	69	2	53	38	15
Moro ...	...	20	18	2	27	18	9
Junker ...	...	121	116	5	21	15	6
Schenek and Seyffert	...	28	27	1	20	15	5
Renschel ...	...	11	11	0	0	0	0
Heinemann ...	...	68	54	14	50	37	13
Feer ...	...	25	24	1	28	14	14
Bandler and Kreibich	...	26	22	4	0	0	0
Von Pirquet ...	...	86	82	4	114	94	20
Kelmar ...	...	25	21	4	20	16	4
Total ...	...	1,607	1,425	182	788	537	251
Percentage	...	...	88 per cent.	22 per cent.	...	68 per cent.	32 per cent.

On the strength of a positive tuberculin reaction alone, we are not justified in making our diagnosis and deciding upon specific treatment, but its true value must be assessed in each case in conjunction with the general and local symptoms. If we proceed thus carefully, the specific reaction will very often prove the most important factor in determining our course of action as regards treatment.

From what I have said about the nature of the reaction, we might theoretically be justified in ascribing a certain amount of prognostic value to this specific diagnosis. In a few cases I found the prognosis, based partly upon the reaction borne out by the subsequent results of the treatment, but further investigations and more proofs will be required before we can more generally accept such an interpretation.

For the ambulatory treatment of pulmonary tuberculosis, two methods appear to me especially suitable—

1. The specific treatment by hypodermic injections of one of the different tuberculin preparations, and
2. The treatment by Bier's passive hyperæmia by means of Kuhn's suction mask.

In addition, hygienic-dietetic measures will, of course, be adopted if possible.

To judge correctly the action of tuberculin, we have to bear in mind that it is not directly curative, like diphtheria antitoxin, but, when introduced into the organism, stimulates the living cell to form anti-bodies. This requires, naturally, a certain amount of energy, and if such energy be missing, we cannot expect the tuberculin treatment to cure. But, even in advanced cases, with a low temperature, the beneficial effects of tuberculin injections are often very remarkable: the difficulty in breathing, fever, night sweats, and the pains in the chest disappear, the distressing cough and the expectoration become less, or stop altogether, the appetite returns, and an all-round improvement apparently takes place. Unfortunately, in the great majority of such cases, it is only of a transitory nature.

In the past, fever, heart diseases, hysteria, neurasthenia, and epilepsy have been considered contra-indications for tuberculin injections, but if the initial doses are very small— $1/100$ to $1/1000$ mgr. of Koch's tuberculin (old)—these complications are no obstacle to the treatment.

Pregnant women stand it exceedingly well.

The tuberculin treatment has to be strictly individual, and a reaction, recognised by a sudden rise in the temperature, should be avoided. Has a reaction taken place, it is best to wait until all symptoms have disappeared, and then to repeat the last dose; a smaller dose than the previous one should not be given. No difficulties will, as a rule, be experienced if one commences with $1/100$ mgr. of Koch's tuberculin (old). Of other preparations, new tuberculin (T.R.), initial dose $1/10$ mgr., and new tuberculin (Bacillen—Emulsion), initial dose 0.0025 mgr., may be used with the same satisfactory results. It is of importance to remember that the same tuberculin preparation should be employed during the whole course of each treatment.

The estimation of the opsonic index by Wright's method can hardly be done in ambulatory practice.

The duration of a course of tuberculin treatment should be about six months, but there is no reason why it should not be continued much longer. Sahli, for instance, advocates its continuation for two, or even three, years, employing very minute doses of Beranek's tuberculin.

During the treatment, the patient's temperature should be taken four times a day at 8 a.m., noon, 4, and 8 p.m.

The number of cases treated by me recently is too small to be used for statistical purposes, and I have further experienced great difficulty in inducing the patients to report themselves after the injections had been discontinued, but there can be no doubt that the specific treatment is, at the present time, superior to any other ambulatory treatment of consumptives.

I give the result of eleven cases treated with Koch's tuberculin (old) in ambulatory practice—

Stage.	Number of Cases.	Cured.	Permanently Improved.	Improved.	Unchanged.	Worse.	Died.
I. ...	3	2	1	...	...	...	...
II. ...	5	1	2	1	1	...	...
III. ...	3	...	...	1	...	1	1
Total ...	11	3	3	2	1	1	1
Percentage	...	27%	27%	18%	9%	9%	9%

Hammer, of Heidelberg, has used Koch's Tuberculin (old) for the last eight years. His patients belonged to the working classes; they continued mostly to follow their usual occupation during the treatment, and no special hygienic-dietetic measures were taken. Nevertheless, he had success really in all of his cases; in a large number of the 100 patients treated by him until 1906, a cure was effected or a permanent improvement had taken place.

Moeller, of Berlin, gives a very interesting comparison of the ambulatory treatment with and without tuberculin, also of the ambulatory tuberculin treatment and the sanatorium treatment combined with tuberculin injections:—

	Cured.	Greatly improved.	Improved.	Unchanged.	Worse.	Died.	Total.
Ambulatory Treatment without Tuberculin	5=10 per cent.	15=30 per cent.	15=30 per cent.	6=12 per cent.	6=12 per cent.	3=6 per cent.	50
Ambulatory Treatment with Tuberculin	18=36 per cent.	15=30 per cent.	13=26 per cent.	4=8 per cent.	0	0	50
Ambulatory Treatment with Tuberculin	18=36 per cent.	15=30 per cent.	13=26 per cent.	4=8 per cent.	0	0	50
Sanatorium Treatment with Tuberculin.	20=36.3 per cent.	17=30.9 per cent.	14=25.4 per cent.	4=7.3 per cent.	0	0	55

The treatment of phthisis by *Passive Hyperæmia* is not new. As far back as 1835 Ramage, of London, who had noticed the rare occurrence

of pulmonary tuberculosis in asthmatics, made his phthisical patients to breathe methodically through a narrow tube in the mouth; his results have been very remarkable.

It is well known that persons suffering from heart disease combined with pulmonary congestion rarely contract phthisis. This fact has been borne out by 143 autopsies which Rokitansky made on people with hypertrophic hearts; only fifteen of these showed traces of extinct tuberculosis of the lungs, while, as a rule, in about 80 per cent. of adults such evidence can be found after death. These observations induced Bier to try his method of passive hyperæmia in phthisical patients; by compressing the nose he restricted nasal inspiration, while allowing free expiration through the mouth.

Quite recently, Kuhn, assistant to Professor von Leyden, of Berlin, has devised an apparatus which enables us to cause pulmonary hyperæmia by restricting and regulating nasal inspiration in a natural way. It consists of a mask made of celluloid and divided into two separate compartments, one for the nose and the other for the mouth; it is kept in position by an elastic band, and to ensure a close fit its base is formed by a hollow india-rubber rim, which can be inflated with air. In the nasal part of the mask there is a slit which can be opened or closed by means of a sliding valve. This is the most important part of the apparatus; it serves to increase or decrease the resistance to breathing. There is a second slit also closed with a sliding valve, in the partition, which divides the two compartments of the mask. It can be opened in case of an obstruction in the nose either from a cold or some other cause. Expiration can freely take place through two circular openings, one for each compartment, closed with movable valves, which open and close automatically.

The apparatus can easily be disinfected by placing it in a cold 2 per cent. lysol or 3 per cent. carbolic acid solution, rinsing it afterwards in cold sterilized water.

At the beginning of the treatment, the mask should be worn by the patient for about ten minutes in the morning and afternoon, the time to be increased gradually to an hour. At first, only moderate resistance to free inspiration should be caused, by keeping the sliding valve on the nasal piece fairly open. The patient should be in a reclining position while using the mask, preferably in the open air. If used properly and carefully no ill effects or discomfort will be experienced from its use. The effect of the apparatus is by impeding nasal inspiration to cause increased negative pressure in the thorax and subsequent augmented flow of blood into the lungs by suction. This passive hyperæmia, together with an improvement of the lymph circulation in the lungs, has the tendency to promote cicatrization and the formation of connective tissue, and to cause, in course of time, an anatomical and physiological regeneration of the organ.

Kuhn has further proved, by a large number of observations, that the use of his suction mask increases the number of the red and white blood corpuscles, and of the latter especially the polynuclear leucocytes, also the percentage of hæmoglobin. This, by itself, is undoubtedly of great assistance in the treatment of phthisical patients who are mostly anæmic, but if we remember the important part played by the leucocytes in the fight of the organism against the invading bacilli, it is not unlikely that their increase may assist in the formation of anti-bodies.

Von Leyden, Kuhn, Stolzenburg, and other observers, have invariably noticed a beneficial effect upon the general condition of patients using the

mask. Personally, I have seen the respiration, cough and expectoration much improved, a diminution or disappearance of the bacilli, and an increased appetite. The local process in the lungs appeared also to be favorably influenced.

Although the treatment of pulmonary tuberculosis by Bier's passive hyperæmia by means of Kuhn's suction mask is of too recent a date to allow us to form a definite opinion as to its merits or otherwise, I considered the results so far achieved sufficiently interesting to bring them before you. I think the method worth while trying further, and it is certainly eminently suitable for the ambulatory treatment of tuberculosis.

If we compare both the sanatorium and the ambulatory treatment, we find the surprising fact—

1. That the results of the latter do not differ greatly from those of the former.
2. The ambulatory treatment is less expensive than the sanatorium.
3. Its results are probably more lasting, because it can be given for a much longer period.
4. The majority of patients can wholly or partly continue their usual or some kind of occupation.
5. They remain and their improvement takes place under the same climatic and social conditions under which they will have to live afterwards.

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THE VALUE OF THE TUBERCULO-OPSONIC INDEX IN DIAGNOSIS, PROGNOSIS, AND AS A GUIDE TO TREATMENT.

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REMARKS ON THE VALUE OF THE OPSONIC INDEX.

In this paper I do not intend to discuss the opsonic theory or to review the work of others. The practitioners who keep in touch with current literature already know full well from the writing of many enthusiasts the results which are obtained by inoculation methods controlled by opsonic count; and at present the attitude taken up by the practitioners seems to be either that of the Levite of old, "who passed by on the other side,"

or else that of the man who wishes to be "up to the times," and seizes on any new thing and runs it to death or else uses it as a sort of *dernier ressort* and becomes constantly disappointed with results.

A subject like this must be approached with the greatest care and consideration on the part of the practitioner and the bacteriologist, or else it seems to me that there is a great danger of the whole treatment falling into disrepute.

The Value from a Diagnostic Point.

I have been called in upon repeated occasions to "do an index" and make a diagnosis in a case where a question arises as to the exact condition of the patient, and it is to try and throw some light on the diagnostic value of the indices that I am writing this paper. First of all I would like to make it clearly understood that the index—particularly to tubercle—varies, and varies greatly under different conditions. In general or systematic infections one has an index that fluctuates from day to day, in fact from hour to hour; rest, sleep, exercise, temperature, all effect the index. Last and not least, there is the personal factor, details of technique, pitfalls which waylay the hurried or careless investigator at every step. All these factors tend to show that the diagnostic value of one single index may be of very little use and is often misleading.

It seems to be that the index is merely one point in the so-called opsonic curve, and that until such a curve is established very little value can be placed on a single count. On the other hand, with a patient in bed and a fairly constant rise and fall of temperature a great deal of light may be thrown on the diagnosis of obscure cases by the estimation of the index combined with other bacteriological investigations.

Localised infections show the most consistently low indices, whilst systematic or general infections show high and fluctuating indices, the cause of the fluctuations being auto-inoculation.

The Value in Prognosis.

Patients leave the sanatorium having put on weight and improved up to a certain point. They are told that the lungs have dried up, no moisture is present, and that there are no tubercle bacilli present in the sputum or "only a trace." But how often do we find that they quickly relapse in spite of care and continuation of the 'outdoor treatment.' I have examined a number of these cases lately and find a low index to *B. tuberculosis* without much fluctuation, excepting in those cases where bacilli are very numerous in the sputum. Now it is in these late cases—together with the very early cases where the index is low and constant, we can do most good in the tuberculin treatment of tuberculosis of the lungs.

I do not advocate the use of tuberculin where the index is already high and fluctuating, but after the patient has improved with absolute rest and fresh air treatment, and the moisture in the lungs has disappeared; then, if the index is taken, and a curve established which shows very little variation, and is below normal, tuberculin treatment controlled by the index will be one of the main factors in assuring complete recovery.

To illustrate this—case No. 4—who had been under treatment in the sanatorium, and when first seen by me had a high and very variable index brought on, evidently, by auto-inoculation from a focus in the lungs. Rest in bed and milk diet brought index down to subnormal and fairly constant.

Has since been under treatment with tuberculin and shown marked signs of improvement clinically as well as by the increased height of index above normal.

In three other cases, all with low indices and bacilli in sputum, who have undergone treatment in sanatorium for varying periods, the index has been artificially raised by interspaced doses of tuberculin and an all-round improvement is noted.

The Value as a Guide to Treatment.

The necessity of estimation of index is fourfold—

- (a) To regulate the dose of tuberculin.
- (b) To avoid dosing in negative phase.
- (c) To avoid breaking in on the positive phase.
- (d) To avoid postponing dosing after positive phase is over and allowing patient to fall back.

It is frequently urged that the clinical symptoms of the patient should be taken as a guide to the times and strength of the dosage of tuberculin, or that once having taken the index inoculation can be carried out every two or three weeks without any further estimation. Such is not the experience of the writers on the subject, and I have seen very contradictory results in cases so treated. In many cases the clinical symptoms are altogether misleading, and if we are able to get the maximum benefit out of the treatment of tuberculosis by tuberculin, the index should be our sole guide as to the frequency and strength of dosage.

To sum up I take it that the tuberculo-opsonic or rather the opsonic curve—as the index is merely one point in the curve—is of great value in diagnosis and prognosis, and also *is the guide* in the treatment of tuberculosis by tuberculin.

DISCUSSION ON TUBERCULOSIS AND ITS TREATMENT AND THE VALUE OF OPSONIC ESTIMATIONS.

DR. SPRINGTHORPE (Melbourne) said:—(1) That he had used the old tuberculin ever since it was originally introduced, and had from time to time published the results of his work, by which it would be seen that the effects produced were satisfactory. (2) That he did not think that opsonic estimations were necessary, for the clinical picture and general reaction were a quite sufficient guide as to the therapeutic use of tuberculin.

DR. STAWELL (Melbourne) said:—(1) That Wright's work and his opsonic estimations had been the means of re-establishing the use of a discredited agent, tuberculin, and it was solely by means of the opsonic work that the methods and dosage in the therapeutic use of tuberculin had been indicated; further than this, Wright's work had presented some of the underlying features of the mechanism of immunity to the medical profession in a striking and practical way. (2) That whatever was the ultimate value of tuberculin, Wright must receive the credit for work done which was of profound and lasting value. At present opsonic estimations were of value in doubtful cases both as an index to treatment and as diagnostic signs: he would not say that opsonic estimations were now necessary in the bulk of cases suitable for tuberculin treatment. (3) The ultimate value of tuberculin was to be decided by clinicians, especially those who had the opportunity of sanatorium work. Wright's own clinical experience was largely confined to surgical tuberculosis, and in that he

had won great triumphs. Whether these triumphs could be won in pulmonary tuberculosis was for them to keep on testing and trying. (4) Whether large doses with the excessive local vascular reaction which followed its use would in limited localized lesions of pulmonary tuberculosis be better than very small graduated doses was a point among others which clinical practice might settle within a few years.

DR. JULIAN SMITH. DR. MILLS. DR. MCINTYRE SINCLAIR, and DR. SINCLAIR GILLIES replied.

ANÆSTHETICS.

PAPERS AND DISCUSSION IN THE SECTIONS OF MEDICINE AND OF SURGERY.

THURSDAY, 22ND OCTOBER, 1908, 2 P.M.

DR. B. POULTON. President of the Section of Surgery, in the Chair.

THE PRESENT POSITION OF SPINAL ANALGESIA.—AN EXPERIENCE OF 278 CASES.

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The use of a general anæsthetic for the production of surgical analgesia is always a source of possible danger to the patient. Since the introduction of aseptic principles, the risk from the actual surgical interference has been reduced to such an extent that, in a number of operations, the anæsthetic must be regarded as the most dangerous factor. This is especially the case if we take into account, not only the immediate dangers of the anæsthetic, but also any of its after-effects, which may lessen the chances of the patient's recovery.

It is the recognition of these disadvantages of general anæsthesia that has led to the introduction and perfection of various methods of local analgesia. But, useful as these methods are, they, unfortunately, have their limitations, and a class of cases still remains in which the risk of a general anæsthetic is so great as to preclude its use, and where it is not practicable to employ local analgesia.

The introduction of spinal analgesia seemed to furnish a method that would not only prove an efficient substitute in a number of these cases, but would also be preferable to a general anæsthetic in conditions where the use of the latter is not absolutely contraindicated.

But although during the last ten years surgeons have made extensive use of this method, and have fully reported their results, both favorable and unfavorable, there is still a great diversity of opinion, not only as to its proper position amongst the recognised forms of producing analgesia, but also as to the best drug to be used, and the method of its administration.

It is a question of great importance to determine whether experience justifies the use of spinal analgesia, and, if so, the class of cases in which it should be given preference to other recognised methods. To solve this question, it is necessary to carefully consider not only the advantages, but particularly the disadvantages that are associated with this form of producing analgesia.

In addition to the small degree of uncertainty of obtaining an efficient analgesia, there are a number of undesirable effects that either accompany or follow the spinal analgesia, and form the chief disadvantages of this method. Of such are the nausea and vomiting, the collapse, and the temporary paralysis of the muscles of respiration, that have occurred during the analgesic stage.

Among the after-effects, the most frequent is severe and often distressing headache, frequently associated with pain in the back and limbs, and occasionally accompanied by rigidity of the back and neck. There occur, in addition, rise of temperature (in a small percentage of cases) and a transient nephritis. But the most serious of all the after-effects is the occurrence of the various muscular paralyses. As a rule, such paralysis affects the ocular muscles, chiefly the external rectus, but also the superior oblique, and occasionally those supplied by the oculomotor nerve. Paralysis, however, has affected the muscles of the lower extremities, usually those of the peroneal group. The sphincters of the bladder and rectum have also been involved.

Fortunately, the occurrence of these paralyses has been very rare, and, as a rule, the paralysis has been of a temporary nature, and has ended in complete recovery. In one case, however—the much-quoted case of König—the complete paraplegia which followed the intra-spinous injection led to a fatal issue.

Different writers vary considerably as to the frequency of these ill-effects, and I will therefore briefly refer to their occurrence in 278 cases in which I have administered this method. In these cases, wherever possible, lumbar-puncture was performed in the median line, through the second lumbar interspace, with the patient in the sitting position, and bending well forward. In all the cases, except in those where the analgesic was dissolved in the cerebro-spinal fluid, a quantity of this fluid was allowed to escape. The actual amount—from 6 to 10 cc.—depended on that of the analgesic solution, and on the pressure of the escaping fluid. Where it was desirable to obtain a high analgesia, the pelvis was elevated immediately after the injection, and was lowered as soon as the resulting paræsthesia had reached the required height.

Stovaine was employed in 249 of the cases, novocaine in 24, and tropacocaine in 5.

In 195, the stovaine was administered in the form of a 1 per cent. solution in normal saline. In 20, it was given dissolved in the withdrawn cerebro-spinal fluid, and in 24 in the form of a 1 per cent. solution of stovaine dissolved in a 5 per cent. solution of glucose.

The novocaine and tropacocaine were administered in the form of a 2 per cent. and a 1 per cent. solution respectively in normal saline.

The dose of stovaine employed was grain $\frac{3}{4}$ to grain j. in perinæal and anal operations, and grain j. to grains $j\frac{1}{4}$ for operations on the pelvis, abdomen, and lower extremities. In one or two cases up to grains jss. of stovaine was used without any apparent bad effect.

Of novocaine, the dose employed was grains jss. to grains $ij\frac{1}{4}$, and of tropacocaine just less than grain j.

Adrenalin m. iij. to v. of a 1 pro mille solution was added in those cases where it was necessary to obtain a long period of analgesia, and in all but one of the operations on the bladder.

The operations performed included practically all those that are performed below the trans-pyloric plane, as well as several gastro-enterostomies. The age of the patients ranged from 17 to 86.

On four occasions I failed to perform the necessary lumbar puncture.

In 20 cases the resulting analgesia was imperfect either in its degree or its extent, whilst in five (including two Kraske's operations) it was insufficient as regards its duration. In these it was necessary to employ a general anæsthetic, either during or at the end of the operation.

In two cases the spinal analgesia was purposely combined with general anæsthesia to eliminate the risk of reflex shock.

The duration of the operations was from 15 to 100 minutes, except in one for the removal of a large pelvic tumour, which occupied 2 hours and 40 minutes, and where a perfect analgesia obtained throughout the operation.

As regards the occurrence of headache, backache, and pains in the limbs following on the administration of the analgesic, my results are unfortunately, not nearly as favorable as those reported by a number of observers, who only refer to their occasional occurrence. The one or the other has been noted in just over 50 per cent. of the cases, and in some of these has been distressingly severe and persistent. Nor have I been able to account for this by any difference in the method of administration, or in the analgesic employed.

Although tropacocaine was only used in five of the cases, one of the worst headaches occurred in one of these, and the cerebro-spinal fluid withdrawn 24 hours after the injection was quite turbid, and yielded a copious puriform deposit, consisting mainly of polynuclear leucocytes. A bacteriological examination showed the fluid sterile.

Marked collapse occurred in six of the cases, and in all of these towards the end of the operation. It is, however, somewhat difficult to exactly estimate the importance of this, for in all but one of the cases there was present some circulatory or other condition which predisposed the patient to such an event.

In three of the patients the intra-spinal injection was rapidly followed by a complete temporary paralysis of all the respiratory muscles in the one, and of the intercostal muscles in the other two cases. Stovaine-adrenalin solution in normal saline was used in two of these, and stovaine-glucose solution in the other. In all three, the stovaine was administered with the patient in the sitting position. In the case that received the stovaine-glucose solution, and which developed a complete intercostal paralysis, the pelvis was not elevated subsequent to the injection; in the other two it was, but in both of these some delay occurred between the raising of the pelvis and the determination of the upper limit of the resulting paræsthesia. In one of these last two cases a complete respiratory paralysis developed within three minutes of the injection, and it was necessary to perform artificial respiration for a period of 35 minutes, when the diaphragm resumed its function; this kept the patient comfortable, and did not interfere with the performance of the operation—a supra-pubic prostatectomy. The other two patients soon accommodated themselves to their diaphragmatic respiration without artificial aids.

As bearing on the limitation of the analgesia and accompanying muscular paralysis that follows the intra-spinal injection of stovaine, I have

repeatedly observed a hyper-algesia as the first noticeable sensory change; this hyper-algesia rapidly passes into a hyp- and ultimate analgesia, the limit of the last accurately corresponding with that of the first.

Two of the patients developed a paralysis of the external rectus of the eye. Both received gr. $j\frac{1}{4}$ of stovaine and m. iv. of a 1 pro mille solution of adrenalin as a 1 per cent. solution in normal saline. In both, the injection was followed by severe and persistent headache. In one, the paralysis was of the usual transient nature, and ended in complete recovery after a few weeks. In the other, it still existed a year after the operation.

By far the most serious complication occurred in a male patient, æt. 29 years, who, on the 6th December, 1906, received gr. j. of stovaine and m. iv. of adrenalin in the form of a 1 per cent. solution in normal saline. The injection was made through the second lumbar interspace, and nothing unusual was noted in performing the same. The injection was followed by severe headache, backache, and vomiting. For the first four days the patient suffered from retention of urine, and this was followed by a difficulty of control over bladder and bowel. Towards the end of January, 1907, he complained of numbness and weakness in the right leg, and examination revealed a paralysis of the external popliteal, corresponding to the fifth lumbar segment. Notwithstanding massage and electrical treatment, this gradually developed into a typical spastic paraplegia. Under anti-specific treatment, however, he has regained complete control over bladder and bowel, and his condition has made considerable improvement.

Such, then, are the disadvantages of spinal analgesia. Amongst these I have purposely not included the absence of the loss of consciousness. It is, at the least, debatable whether the disadvantages of this in some cases is not outweighed by the advantages in others. There are some that dread the loss of consciousness more than the operation. Only in one case have I noted any disadvantage as a result of it, viz., an interference with the performance of the operation; this, moreover, was in a man.

As regards the advantages of spinal analgesia, there is the almost entire absence of reflex shock. This is particularly noticeable in operations on the pelvis, rectum, perinæum, and lower extremities. It is of special importance in the old and debilitated, where the presence of such a factor may mean all the difference between success and failure. In this class of cases the absence of vomiting, and the patient's ability to take food and drinks immediately after the operation, is of no little importance in increasing the chances of the patient's recovery.

There is, further, the advantage of the greater safety with which this method can be efficiently employed in operations on the lower abdomen, pelvis, perinæum, and lower extremities in alcoholics, diabetics, and those suffering from severe cardiac and pulmonary lesions.

The muscular and sphincter paralysis and relaxation is also a distinct aid in operations on the rectum, the bladder, and the lower abdomen. In some of these the paralysis of the sphincter, moreover, eliminates the small degree of additional risk associated with its dilation during general anæsthesia.

The complete muscular relaxation which obtains with stovaine and novocaine analgesia is an important help in the repair of musculo-aponeurotic defects of the abdominal wall, and in dealing with fractures. In this class of cases the absence of vomiting after the operation, and the consequent absence of strain, is occasionally a factor in insuring a more perfect result.

Lastly, in operations on the prostate, there is a lessened liability to hæmorrhage—the result, no doubt, of the absence of reflex shock, and the combined tonic effect of the stovaine and adrenalin on the vasomotor system.

It is the opinion of competent observers that, in a number of the series of cases that form the basis of this paper, one or other or a combination of the above factors has been responsible either for the actual saving of the patient's life, or for an increase of the chances of his recovery.

Spinal analgesia, then, as at present produced, possesses undoubted advantages as well as serious disadvantages. As regards its disadvantages, some are undoubtedly the results of imperfection in the technique, and therefore capable of elimination. As regards others, it is important to determine the part that is played in their production—

- (1) By the drug that is employed, and its dosage.
- (2) By the concentration and character of the solution in which the analgesic is administered.
- (3) By the condition of the spinal cord of the patient, and the condition of the patient generally.

In reference to technique, it is noteworthy that most workers have experienced considerable improvement in the results of their later cases. There is evidence, moreover, that such accompanying effects as nausea and vomiting, respiratory muscular paralysis, and collapse are, in a number of cases, the result of a faulty technique. Wolff's* report of the occurrence of an external rectus paralysis, after simple lumbar puncture, suggests that even this complication may be an effect of the technique rather than of the drug injected.

As regards the drug, it must be admitted that the ideal spinal analgesic has yet to be discovered, viz., one that will give us the transient analgesic and paralytic effects of stovaine without its irritating and toxic after-effects. Of the three drugs—stovaine, novocaine, and tropacocaine—now in use, there is little to choose as regards their liability to produce headache, backache, and collapse. Of these, however, stovaine is the most efficient analgesic. Tropacocaine has, clinically, little action on the motor cells, and it is therefore probably less likely to produce pathological effects in this direction. On the other hand, the muscular paralysis and relaxation accompanying the use of stovaine and novocaine is absent in tropacocaine analgesia.

As regards the dose: It is suggestive that, in the few cases where it has been possible to make a microscopic examination of the spinal cord of stovanised persons, (Spielmeyer),† changes in the anterior horn cells were only demonstrable in those where a large dose (gr. 1.54 to gr. 1.85) of stovaine had been employed.

As to the concentration of the solution, it seems reasonable that the results of the researches of Reclus should also hold good in the employment of such drugs as stovaine, novocaine, and tropacocaine inter-spinally.

Concerning the part played by the condition of the spinal cord: It is possible that this may increase or diminish the susceptibility of the cord to the action of the analgesic drugs, not only in the direction of the resulting analgesia, but also in that of pathological changes. It has been

*Wolff, *Eerliner Klinische Wochenschrift*, October 14, 1907.

†Spielmeyer, *Münchener Medizinische Wochenschrift*, August 4, 1908.

suggested that in syphilis the intra-spinal injection of stovaine or novocaine may play a similar part in the production of a spinal specific lesion as that which is occasionally played by an injury in the production of a gumma.

Further experience with this method will, no doubt, enable us to definitely determine conditions of the spinal cord and of the patient generally, in which the use of spinal analgesia is contra-indicated. Even now spinal analgesia must be given a definite, though limited, position amongst the recognised methods of surgical analgesia. That it is a method worthy of further investigation must, I think, be admitted. To what extent it will eventually further supplement or replace the use of general anaesthesia will, in the main, depend on such advances as well lead to the reduction of its disadvantages.

NOTES ON CASES UNSUITABLE FOR CHLOROFORM ANÆSTHESIA.

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Some few months ago, three (3) deaths from chloroform occurred in Victoria, within a few days of one another, all of them young women in a practically normal condition of health. There was not the slightest doubt but that all died from chloroform poisoning, a *post-mortem* and inquest being held in every case.

Two of the patients died before the operation was commenced, and, in the third, the operation was a very slight one.

These three striking cases made one recall others of a somewhat similar nature, which, although fortunately not dying under chloroform, at least went so nearly doing so as to cause the gravest anxiety to be felt at the time, and the confidence of the anaesthetist to be rudely shaken.

It was *then* recognised that such a condition of things could not go on without a fatality being recorded sooner or later, and a search was instituted to find out why this class of case should go so suddenly and completely wrong, and, if possible, to devise a means of preventing such accidents in the future.

Attention was first directed to the blood pressure, on the supposition that with a low existing blood pressure chloroform caused the blood pressure to fall so much further as to cause death.

Experiment showed that chloroform *lowers* the blood pressure in practically every case, but it was soon demonstrated that the blood pressure of young women was not usually low, averaging from 110 to 130 hgeum, and still, even with a high-blood pressure, these young women did not do well under chloroform.

It was observed, however, in women of maturer years (45 or over), where the blood pressure was not more than 100 hgeum, that chloroform could be given without any untoward symptoms occurring. The theory that a low-blood pressure was the cause of the dangerous symptoms had then to be abandoned as fallacious, and some other cause sought.

The next observations, instead of being directed to the pressure of the blood, dealt with the blood itself, and it was found that wherever any

degree of anæmia existed, no matter what the age of the patient might be, nor what the blood pressure might read, chloroform produced dangerous effects on the circulation, and the greater the degree of anæmia that was present, the more pronounced the effect on the circulation.

The cause of the anæmia did not seem to matter, whether it was a chlorosis in a young woman from physiological causes, an anæmic condition in an old woman from repeated hæmorrhages from a malignant uterus, anæmia from sepsis, or from any other cause. So long as anæmia was present, chloroform affected the circulation, and dangerous symptoms appeared early in the administration, quite irrespective of age.

Now, in anæmia, there are several factors to be considered—

1. The total volume of the blood is increased, whilst the relative number of its corpuscles is diminished.
2. The larger quantity of the blood circulating allows more chloroform to be absorbed into it.
3. The susceptibility of the normal heart muscle to chloroform poisoning is such a well-known physiological fact that it is scarcely necessary to mention it, but when the nutrition of the heart is interfered with by the poverty of the blood, it is still less able to withstand the action of the chloroform, and rapidly becomes poisoned and paralysed by the direct action of the drug upon the heart muscle.

When these facts became established, chloroform as an anæsthetic in cases of anæmia was discontinued, and ether, by the closed method, substituted. The results proved satisfactory in all cases. The ether was well tolerated, no bad symptoms presented themselves, and a very small percentage of ether was proved to be sufficient to produce safe and continuous anæsthesia.

The conclusions arrived at on the consideration of these clinical facts are—

1. That it is unsafe to administer chloroform in all cases of anæmia ;
2. But that ether is safe, and, as an anæsthetic, as efficacious as chloroform, and all anæmic patients take it well.

When ethyl chloride came once more into use as a general anæsthetic, the ethyl chloride and ether sequence was substituted for ether alone, and the results again proved most satisfactory, no dangerous symptoms ever manifesting themselves.

The advantage of the ethyl chloride ether sequence is that it is very pleasant to inhale, it is very rapid in producing anæsthesia, it is safe, there is scarcely ever any struggling, never any talking, and, as less ether is required, cyanosis and hyper secretion of mucous are much diminished, and it is strongly recommended that every patient requiring an anæsthetic should have anæsthesia induced by this method, excepting children under four (4) years of age, midwifery, and special cases.

If, however, during the operation the ether is not well tolerated, chloroform can then be substituted with much less risk than obtains when it is used as a preliminary anæsthetic, for nothing is more certain that with the routine indiscriminate use of chloroform for every case that comes along, trouble is sure to be met as soon as anæmia is encountered, and a death occurs, which, had other means been adopted, could have been avoided.

The data on which these remarks are based are taken from 900 recorded consecutive cases at the Women's Hospital.

LOCAL ANÆSTHESIA IN OPERATIONS ON CHILDREN FOR EMPYEMA.

SYDNEY W. PATTERSON, M.D.

Operation for empyema under general anæsthesia is usually unsatisfactory. The surgeon prefers the patient to be in a position which the anæsthetist considers extra hazardous. The patient is necessarily lightly anæsthetised; and the operation is hurried, so that proper, that is, the gradual and steady, re-expansion of the collapsed lung is not obtained.

For over two years, empyemata at the Melbourne Hospital for Sick Children have been drained under local anæsthesia by the method described below.

Complete local anæsthesia is obtained. Resection of a portion of rib—which is always necessary to secure efficient drainage in children—can be carried out without the least consciousness of pain on the part of the child.

Older children have frequently volunteered the statement on leaving the table that I did not hurt them as much as the doctor who stuck the needle in when exploring the chest.

The operation can be carried out in a leisurely manner; the pus being allowed to drain away slowly, so that the lung re-expands simultaneously with the evacuation of the fluid—slowly, steady, and gradually—and the risk of cardiac embarrassment is reduced to a minimum.

Even small children lie quite quietly on the table if amused by a nurse or by the "patter" of the operator. The assistance of an experienced anæsthetist is unnecessary; a consideration obviously of value in a country practice.

Since May, 1906, 72 cases have been operated on by this method at the Children's Hospital; the youngest being a baby of four months.

In one case, a right empyema was drained, and, three weeks later, pus from the left chest was also evacuated in the manner described.

ANÆSTHETIC.

I. In hospital practice, the anæsthetic used is a solution of eucain gr. 9/10, sod. chlor. gr. 3 3/5 in 5 i. of water, sterilized by boiling. Ten minims of adrenalin (1 in 1,000) are added to each ounce of solution just before use.

II. For ordinary use, in private practice, 3 grains of B-eucaine lactate and one sodium chloride tablet (No. 365, Parke, Davis, and Co.) are boiled in a 100 cc. (5 iii. ss.) beaker of water; the solution is cooled and 1 ampoule (1/2 cc., P., D., & Co.) of adrenalin added.

METHOD.

The chest having been prepared in the usual way, the child is placed on the table. For a basal empyema, the patient lies on his good side, with one arm underneath the body, and the uppermost arm held forwards and upwards towards the child's head by a nurse. The site for incision is, of course, over the point at which exploratory puncture has discovered the pus; the site of election for basal empyema is over the ninth rib in the scapular line.

The skin is anæsthetized for 2 inches along the course of the rib chosen for resection by injecting the anæsthetic with a hypodermic syringe, and

lifting the superficial from the deep layers of the epidermis. After the first needleprick nothing more is felt, as each extra injection is introduced through a previously anæsthetized area. The subcutaneous tissues and muscles are infiltrated with the solution from a large serum syringe; great quantities of this anæsthetic being used without ill effect. The incision is made almost at once, and carried through the muscles. The external periosteum is full stripped off the rib with a Wood's angled raspatory ballooned off the rib by the solution. It is incised longitudinally along the portion of rib to be resected, and transversely at each end of the longitudinal line; then it is raised by an ordinary periosteal elevator. A syringe having a fine full-curved needle is used to anæsthetize the internal periosteum. The needle being passed first round the upper border of the rib, then round the lower, between the periosteum and the rib, the solution is forced between the internal surface of the rib and the periosteum. The periosteum is full stripped off the rib with a Wood's angled raspatory (made by Down, London; a cleft palate instrument originally). The exposed portion of rib is resected with bone-cutting forceps. The parietal pleura is then swabbed with 10 per cent. cocaine solution, and a scalpel is pushed through it gently.

The forefinger being used to form a valve, the pus is allowed to escape with expiration, and the opening is closed during inspiration to prevent air being sucked into the cavity.

Frequently the incision in the pleura must be enlarged to allow big flakes of lymph to come away.

The patient is turned on his back to allow freer drainage. The pus is gradually evacuated in this way, and the lung can be felt to expand and to come to the bottom of the wound.

When the chest is nearly emptied an attack of coughing may occur; then the wound is closed by the finger completely, and the paroxysm quickly subsides.

When the empyema is evacuated, the patient is turned on his side again, a drainage tube is inserted and fastened round the chest with tapes. The incision is partially closed with silkworm gut and horsehair sutures; dressings and a binder with shoulder-straps are applied, and the patient is returned to bed.

GENERAL ANÆSTHESIA IN THE LIGHT OF RECENT PHARMACOLOGICAL RESEARCH.

E. H. EMBLEY, M.D.

Whilst conferring upon suffering humanity one of the greatest blessings, general anæsthesia has been attended, in exceptional instances, by fatal results. For many years the anæsthetist has called for more light as to the causation of such fatalities and for some means of their prevention. Observations made of the phenomena attending clinical fatalities failed to furnish adequate interpretation. This arose in consequence of the multiplicity of factors concerned, all of the co-ordinating factors engaged in the maintenance of the circulation being more or less involved; the resultant depression being an algebraic sum of the several effects. The interpretation of such a complex problem was possible only by means of the results of experimental investigation of the action of the anæsthetic agents upon each of those factors taken separately.

This paper is an endeavour to concisely emphasize the results of recent pharmacological research, only, however, so far as it affects the important practical questions which the anæsthetist has called for solution.

During the performance of such experiments it was found that the phenomena which ensued upon the administration of the anæsthetic agent were common to all animals employed, but that variations in degree of the different effects occurred with different species of animal, and to a less extent with different animals of the same species. The pharmacodynamic effect upon the heart and arteriolar muscle appeared to be the same, both in degree and kind for all animals. That upon the vagus, vaso-motor and respiratory nervous mechanisms varied in degree but not in kind. In man the effect upon heart and arteriolar muscle is doubtless the same, both in degree and in kind, as that of the lower animals; whilst interpretation of clinical phenomena in the light of experimental results strongly supports the assumption that the effect upon the vagus, vaso-motor and the respiratory nervous mechanisms are the same in kind, but that they vary in degree in different individuals, as they do in different species and different animals of the same species.

All recent research in general anæsthetics has practically been confined to chloroform, ether, and ethyl chloride. My remarks will therefore be limited to these. All references to ether, unless otherwise stated, will mean ether administered by the open method as distinct from ether administered by the closed method, such as Clover's, which is ether plus mild asphyxia.

The fall of blood pressure indicative of failure of circulation is of two kinds—

1. The common fall, which is steady and regular.
2. The syncopal fall, which is uncommon and sudden in onset.

The common fall of blood pressure occurs equally readily with all these anæsthetic agents when administered in narcotic percentages. When it occurs within the limit of time occupied by an ordinary operation, it results from myocardial and peripheral vascular depression. That from chloroform and ethyl chloride being mostly myocardial, whilst that from ether is mostly from peripheral vascular depression. That which occurs in the prolonged administration of either of these agents is contributed to by the oncoming of central vaso-motor depression.

The syncopal fall of blood pressure occurs much more commonly during ethyl chloride and chloroform narcosis than during that from ether. It results from vagus inhibition of the heart, vaso-constrictor inhibition, or from both. Such effects are due to the direct action of the anæsthetic agent upon the vagus and vaso-constrictor central mechanism or to reflexes from peripheral stimuli of the surgical procedure.

In anæsthetic percentages, chloroform depresses the heart's capacity for work 51 times more than does ether, and nineteen times more than does ethyl chloride.

The effects produced by anæsthetic agents upon the contractility and excitability of heart muscle is the measure of relative safety of such agents. The irritability of the vagus mechanism may be so exalted as to arrest the heart; the vaso-constrictor mechanism may be inhibited and a syncopal fall of blood pressure ensue, and the respiration may be arrested for three minutes or more; but if the excitability and the contractility of the heart be not seriously depressed at the time, recovery always ensues upon the adoption of restorative measures.

Upon the vaso-motor nervous mechanism the effect of chloroform and of ethyl chloride is primarily one of stimulation lasting a relatively long period. This is followed by a slowly oncoming depression. That of ether is not accompanied by a period of stimulation and the subsequent depression is later in onsetting than is that of chloroform or ethyl chloride.

The stimulation of the vaso-motor mechanism by chloroform and by ethyl chloride is usually pressor, occasionally it is depressor, *i.e.*, vaso-constrictor inhibition, especially from that of ethyl chloride. There appears to be no evidence, either clinical or experimental, of such depressor effects in ether narcosis.

All of these anæsthetic agents produce depression of the neuromuscular tone of the arterioles. This is usually more marked in the case of ether in which there is, as above stated, no primary counteracting stimulation of the vaso-constrictor mechanism such as usually occurs from chloroform and ethyl chloride.

The irritability of the cardio-inhibitory mechanism is first raised and then depressed by all three anæsthetic agents. This exaltation of vagus irritability is a marked feature of the narcosis of both chloroform and ethyl chloride. It is much less so in that of ether. The onset of depression is more rapid in ether than in chloroform or ethyl chloride narcosis, and more rapid in chloroform than in ethyl chloride narcosis.

The respiration is depressed by all three anæsthetic agents, but much more rapidly by chloroform and ethyl chloride than by ether. A period of exaltation occurs before the onset of depression. This period varies in duration for each. It is pre-anæsthetic in case of chloroform, co-incident with the onset of anæsthesia in that of ethyl chloride, for a long period during the anæsthesia of ether, and for a still longer period in the anæsthesia of ether plus mild asphyxia. The exaltation produced by chloroform is frequently preceded or accompanied by periods of inhibition of respiration. Such periods further exalt the rate and depth of respiration following. Respiratory paralysis may occur in the narcosis of all of these agents.

The administration of ether by the closed inhaler, such as Clover's, results in anæsthesia by mixed narcosis, *i.e.*, ether with mild asphyxia. This combination produces pharmacodynamic effects widely different from those of ether alone. It is attended by a higher blood pressure, the result of mild asphyxial stimulation of the vaso-constrictor nervous mechanism. In consequence of the early onset of depression of the cardio-inhibitory mechanism produced by ether, and the well-sustained blood pressure due to the asphyxia, such anæsthesia is free from cardiac-inhibition. It is also free from vaso-constrictor inhibition since that mechanism is in a condition of active pressor stimulation throughout, as has been stated. The respiration, moreover, is very efficient, and its pump action upon the splanchnic bed is in marked contrast with that of chloroform. It is a much safer anæsthetic than ether alone.

Chloroform, ether and ethyl chloride each enter into loose chemical combination with some of the constituents of the blood and tissues. The co-efficient of absorption of the vapour of each is high. The vapour tension in the blood and tissues of either of the agents varies with the concentration of these combinations. The efficiency of contractility of heart muscle varies inversely with the vapour tension in the arterial blood. The body tissues and fluids slowly approximate to the vapour tension in the arterial blood, whilst the venous blood has a vapour tension approximately that of

the tissues and lymph. It gradually rises during the progress of continued administration. The total quantity of anæsthetic agent—more particularly chloroform—in the body at any time has very little clinical interest, since most of the deaths from chloroform and ethyl chloride occur in the induction and the period of early anæsthesia, when the total quantity of anæsthetic agent in the body is very small. Death probably never occurs from chloroform or any other anæsthetic agent as a result of over accumulation in the blood and tissues. The vital question is the vapour tension of the anæsthetic agent in the arterial blood, or otherwise, the tension in the capillaries of the heart and medulla.

In the administration of anæsthetic agents by respiration, as in clinical methods, the vapour tension of the anæsthetic agent in the arterial blood varies directly with that of the air mixture inspired. The sum of the capillary surfaces in the walls of the pulmonary alveoli presents a relatively enormous area for absorption. This circumstance, together with that of the high absorption co-efficient possessed by blood, is the reason why the vapour tension of anæsthetic agent in the passing pulmonary blood current rapidly approaches equilibrium with that of the inspired air. The actual volume of anæsthetic vapour inspired in a given time varies directly with the depth and rate of breathing, and the tension of anæsthetic vapour in the air inspired. If, for instance, the respirations be five times as deep and three times as rapid as the normal—a condition which sometimes occurs during the exalted respiration of the induction period of chloroform administration—the volume of the air anæsthetic mixture inspired is fifteen times that of normal breathing. This would cause a great rise in the vapour tension of the anæsthetic agent in the arterial blood.

When chloroform is administered on the flannel mask, and say 5 per cent. vapour be inspired under the conditions of respiration above mentioned, the rise of vapour tension in the arterial blood is very great indeed. Hence the danger of “pushing chloroform” in the induction period.

The intensity, moreover, of the pharmacodynamic effects does not simply increase *pari passu* with the increments in vapour tension of the anæsthetic agent in the arterial blood, it is more rapid, thus—

- 3.3 per cent. CHCl_3 vapour in the air depresses the heart's contractility twice that of 2.2 per cent.
- 19.1 per cent. ether vapour in the air depresses the heart's contractility 4 times that of 10.0 per cent.
- 26.0 per cent. ether vapour in the air depresses the heart's contractility 9 times that of 10.0 per cent.
- 30.0 per cent. ether vapour in the air depresses the heart's contractility 22 times that of 10.0 per cent.

A triple increase in the volume of ether vapour inspired in a given time may mean a 22 times increase in the intensity of pharmacodynamic effect. The danger of excessive lung ventilation in the administration of chloroform is common to all methods of dosage regulation. It occurs rarely, however, in the clinical use of the percentage or regulating apparatus for chloroform. The regular intake of a low percentage of chloroform vapour which is effected by this class of apparatus, is only occasionally attended by inhibited or exaggerated breathing. It is, in this respect, in marked contrast with that produced by the administration from the flannel mask. It is in consequence of this that the percentage chloroform regulating apparatus has justified its claims of greater safety in its employment. There would not appear to be any great gain in the

employment of an instrument which, whilst delimiting the intake by 1 or 2 per cent. in one direction, admits of an intake fifteen times greater in another direction. The uniformity of the anæsthesia and the freedom from dangerous oscillations in depth of narcosis attained by the use of the various forms of percentage regulating apparatus for chloroform are a revelation when the instruments are first used. The anæsthesia, however, is not free from the occasional occurrence of reflex vaso-constrictor inhibition in consequence of the conductivity of the reflex arcs being less depressed by the light anæsthesia usually attained.

When the vapour tension of chloroform in the arterial blood is raised excessively by the administration of concentrated vapour in the air inspired, or by excessive lung ventilation, or both, the pharmacodynamic results vary with the period at which this excess occurs. Syncope is very possible when it occurs in the induction or the period of early anæsthesia. At this period the vagus mechanism is active, so that whilst the excitability and the contractility of the heart are being rapidly depressed, the irritability of the cardio-inhibitory mechanism is being exalted, and syncope follows as the blood pressure is falling. Vaso-constrictor inhibition often occurs coincidently with the cardiac depression and intensifies the fall of blood pressure which precedes the onset of vague inhibition. Death, of course, may occur from myocardial paralysis alone, as a result of excessive intake of chloroform; but this form of death is rare. The completion of the heart arrest is usually cardio-inhibitory, and the importance of the part played by the cardio-inhibitory mechanism varies inversely with the duration of the anæsthesia. The respiration in cases of syncope from vagus inhibition of the heart, may cease before, but most usually it ceases after, the heart comes to stand-still. When excessive intake of chloroform occurs later in the course of anæsthesia, the respiration usually fails first and syncope usually does not happen; moreover, when the cardio-inhibitory mechanism is thus depressed, the blood pressure may fall nearly to zero without inhibition occurring, and the heart recover with cessation of administration and artificial respiration—should respiration have ceased, which doubtless will have occurred.

Syncope from cardiac inhibition, occurring when the heart's contractility has become much depressed is apt to prove fatal. This applies especially to that caused by chloroform. That occurring in the narcosis of ethyl chloride is less apt to be fatal, because it usually happens before the heart's excitability has become much depressed. Syncope occurs very rarely during the administration of ether, as has already been mentioned. Reflex syncope is less fatal than that due to the direct action of the narcotic agent, because it occurs most commonly when the heart's contractility and excitability have not become greatly depressed. If the excitability and the contractility of the heart are depressed from pre-existing pathological causes, syncope of any kind is dangerous.

In the absence of asphyxia, arrest of the heart by vagus inhibition during narcosis by any of these agents, is improbable when the blood pressure is high. Asphyxia is one of the most effective stimulants of the cardio-inhibitory mechanism. Low blood pressure intensifies the irritability of the cardio-inhibitory mechanism in the same manner as does asphyxia, in consequence of bulbar anæmia.

The conditions favoring the onset of cardiac inhibition, therefore, are—
 (1) An undepressed state of the vagus nervous mechanism, *i.e.*, the induction and the period of early anæsthesia. (2) A low blood pressure.
 (3) Asphyxia. (4) An excessive intake of chloroform or ethyl chloride.

(5) Abnormal excitability of the vagus nervous mechanism, observed clinically as a pathological condition and experimentally as a physiological one.

A combination of these conditions enhances the probabilities accordingly. Asphyxia, for instance, occurring in early anæsthesia following excessive intake, is more apt to be followed by syncope than are any other conditions. What has been said with regard to asphyxia does not apply to ether. The depression of the cardio-inhibitory mechanism in ether administration is so rapid that a combination of all of the above-mentioned factors would be required to raise it. Clinically, it would appear that cardio-inhibitory mechanism of children is rapidly depressed, whilst that of the aged is not readily exalted. In those exhausted by pain or disease, the excitability of both the vaso-motor and the cardio-inhibitory mechanism, as well as the conductivity of the reflex arcs, have become so depressed that syncope is rare. Old men with evidence of marked cardio-vascular degeneration, particularly when this is due to syphilis, often struggle violently during induction of chloroform anæsthesia. They are prone to syncope in consequence of the low blood pressure due to embarrassment of the feeble heart, more or less depressed by chloroform.

In experimental conditions, vaso-constrictor inhibition occurs readily during both chloroform and ethyl chloride anæsthesia. There is no evidence of its occurrence during that of ether, and it appears impossible for it to occur in that of ether plus mild asphyxia. It is difficult to obtain clinical evidence of its occurrence under any anæsthetic agent. It is probable that vaso-constrictor inhibition precedes the onset of many cases of vagus inhibition, aiding its onsetting by further lowering the blood pressure. Not infrequently, it occurs in chloroform anæsthesia as a reflex phenomenon resulting of the surgeon's stimulation of certain sensory areas well known to the anæsthetist.

Clinically, reflex vaso-constrictor inhibition may merely produce pallor, failure of or shallow respiration, diminution in volume, and—except in cases with dilated and degenerated heart walls—an increased rate of pulse. With the incidence of cardiac inhibition the pulse becomes slow or absent. Whether death will result from reflex syncope depends upon the degree of depression of the excitability of the heart at the time when the reflex occurred, be the depression a result of the chloroform or ethyl chloride, or from pre-existing disease or from both.

Vaso-constrictor inhibition may occur at any time during a relatively prolonged period of anæsthesia by chloroform or ethyl chloride. Reflex vaso-constrictor inhibition, however, becomes less possible as the duration of anæsthesia progresses. This is because the conductivity of the reflex arcs varies inversely with the duration of the anæsthesia. It is, hence, unnecessary to maintain deep surgical anæsthesia during the latter part of any prolonged operation. By maintaining light surgical anæsthesia during this period much general after-depression is obviated. Operations in which there is a known possibility of occurrence of reflex vaso-constrictor inhibition under chloroform anæsthesia, might with much advantage be preceded by morphine hypodermically. Vaso-constrictor inhibition does not occur during chloroformization of an animal previously morphinized.

The onset of respiratory failure is precipitated by a fall in blood pressure. The respiration fails with a low blood pressure in the narcosis of chloroform, ethyl chloride, and ether; but with a high blood pressure in that of ether plus mild asphyxia. Chloroform depresses the respiratory

nervous mechanism and deranges the pulmonary reflex adjustment for the systemic respiratory requirements. The shallow respiration of chloroform anæsthesia exerts a relatively feeble pump action upon the bed of the circulation. This circumstance, together with that of the depression of the heart's contractility, accounts for the diminution in the heart's output per unit time. In this respect, the anæsthesia of chloroform is in marked contrast with that of ether plus mild asphyxia, in which the heart's output per unit time is above normal. The respiration in ethyl chloride anæsthesia is more efficient than that of chloroform, less efficient than that of ether, and still less than that of ether plus mild asphyxia.

As has already been stated, when respiration fails in the induction and period of early anæsthesia of chloroform, the heart will be found to have already ceased, and that the liability of the heart ceasing before the respiration diminishes rapidly with the duration of the anæsthesia. When respiration fails, the low blood pressure which precipitated it is still further depressed by cessation of the respiratory pump action and the chloroform in the blood of the pulmonary veins at the time of its cessation. When this blood is disposed of, the heart begins to recover. This occurs if the cardio-inhibitory mechanism be so depressed as to be unable to respond to the stimulus of both the chloroform and the asphyxia. This again, as above mentioned, will depend upon whether the incidence of respiratory failure be early or late in the administration. Recovery of blood pressure will be assisted by asphyxial vaso-constriction if the heart be not too much depressed to respond. A chloroformed heart bears asphyxia as well as an unchloroformed one, provided it be not greatly depressed by chloroform or some pathological condition, and it be not inhibited.

When failure of respiration occurs *after* early anæsthesia, the fall of blood pressure accompanying it, results from the administration of excessively concentrated anæsthetic vapour, or, in case of chloroform and ethyl chloride, from reflex syncope. In the former case, the fault is entirely with the administrator, in the latter case it is outside of the administrator's control.

In chloroform narcosis a moderate degree of asphyxia first excites and then depresses the vaso-motor mechanism, so that apart from the danger of cardiac depression and inhibition, asphyxia greatly increases the after-depression of chloroform anæsthesia.

From what has been stated with regard to the administration of ether by the open method, it may be interred that mixtures of chloroform and ether, or of alcohol, chloroform, and ether are, clinically speaking, simply diluted chloroform. If the ether could in such method, be given in sufficient quantity to produce any pharmacological effect, it would be that of further lowering the blood pressure instead of sustaining it, as some clinicians appear to infer. It may also be well to emphasize the fact that death attributed to the anæsthetic when chloroform is administered, is due to the chloroform and not to impurities in it, since there is no known impurity which is more powerfully toxic than the chloroform itself.

By further diminishing the medullary blood supply, chloroform commonly induces respiratory failure in those morbid conditions associated with compression of the brain. The liability to this occurrence varies directly with the slowness of the heart's rate. It is generally preventable by hypodermically injecting 1/120 grain of atropine fifteen minutes or so before commencing the administration, and by employing as little

chloroform as possible in maintaining anæsthesia. By thus quickening the heart's rate, the blood pressure is raised. This measure should not be adopted, however, unless there is a great probability of the cause of the cerebral pressure being removed, otherwise the after-condition is rendered worse, the natural conservative means having been thrown out of gear.

Destruction of brain tissue in operations under chloroform anæsthesia is attended by rapidly deepening vaso-motor depression. This varies directly with the extent of the destruction and the duration of the manipulations. Retropharyngeal abscess should not be opened under chloroform anæsthesia. With a view to the prevention of syncope, certain drugs may be used. Thus, a hypodermic injection of $1/120$ grain of atropine depresses the cardio-inhibitory action of the vagi; whilst $1/20$ grain of strychnine lessens the tendency to vaso-constrictor inhibition, and $1/4$ grain of morphine both exalts the action of the strychnine and diminishes the liability to reflex inhibition by lowering the conductivity of the reflex arcs. The atropine corrects the tendency of the morphine to depress the respiratory mechanism and of the strychnine to exalt vagus irritability. These drugs may be advantageously used in combination, and hypodermically twenty minutes or so before commencing the administration of chloroform or ethyl chloride. It is unnecessary to use them before the administration of ether for reasons already stated.

When syncope has already occurred, and inversion with artificial respiration has failed to re-start the circulation, direct rhythmic compression of the heart, in addition, should be tried. When the heart is held at a still-stand by vagus inhibition this measure has rarely failed, experimentally, to re-start it. Such measure, however, has never succeeded, in my experience, in re-starting the circulation when it has failed from myocardial paralysis, apart from inhibition. Perfusion of fresh blood through the coronary arteries invariably re-starts a heart so poisoned; but the difficulties in effecting perfusion are such as to render the method valueless in practical anæsthetics.

Clinical cases of shock or exhaustion or those suffering exhaustion from prolonged operation, are in a condition of vaso-motor and cardiac depression; the latter being mostly a result of the low blood pressure in the coronary arteries, a result of the former. In such cases, the intravenous injection of isotonic saline solution is of only temporary benefit. The resulting plethoric rise of blood pressure is rapidly lost, or almost lost, in consequence of the rapid escape of the fluid into the tissues. Still more fugitive in its pressor effect is the active principle of supra-renal gland. Both ergot and strychnia are, however, of great value in such cases, administered hypodermically. The former, by virtue of its constrictor effect upon the neuro-muscular apparatus of the arterioles; the latter, by the stimulus thus given to the central nervous mechanism of the vaso-motor system. The value of strychnine, however, is doubtful in severe shock when the vaso-constrictor mechanism may fail to respond or respond feebly. Physiological rest to the central vaso-motor mechanism is indicated above all in such cases. The action of ergot is independent of the condition of the central vaso-motor system. The intravenous or rectal injection of isotonic saline solution in cases of collapse from hæmorrhage is the most efficient treatment. The rise of blood pressure which ensues from such treatment is sustained. Ether anæsthesia with the patient in the Trendelenburg position should be adopted in such cases.

Clinically, ether, in my opinion, should be the routine anæsthetic agent. Its disadvantages are greatly minimized by proper technique. When so

given, and by Clover's method, it is practically free from all of the dangerous accompaniments of chloroformization. Its greater safety far outweighs any small disadvantages which it may possess. Upon the heart's contractility—

- 3.3 per cent. chloroform is 3.16 times, and 2.2 per cent. is 1.75 times as depressing as 30.0 per cent. ether.
- 3.3 per cent. chloroform is 8.6 times, and 2.2 per cent. is 4.3 times as depressing as 26.0 per cent. ether.
- 3.3 per cent. chloroform is 19.2 times, and 2.2 per cent. is 9.0 times as depressing as 19.1 per cent.
- 3.3 per cent. chloroform is 78.8 times, and 2.2 per cent. is 40.0 times as depressing as 10.0 per cent. ether.

When ether is given by the closed method—Clover's—there is no period of exhalation, no struggling, no inspiration of dangerously high concentrations of ether vapour, no serious cardiac depression, and no vagus or vaso-motor inhibition either direct or reflex. In this closed method respiratory failure is always primary and independent of circulatory depression, and is rapidly restored by artificial respiration. Such occurrence is, however, rare. Moderate asphyxia in ether anæsthesia is beneficial as has been shown. Excessive asphyxia exhausts the vaso-motor system and depresses the heart's contractility, a condition resulting in much after depression. As in the non-anæsthetic state excessive asphyxia may be fatal to pathologically-depressed hearts.

The continuous administration of ethyl chloride in concentrations, from 10 per cent. upwards, is very prone to be attended by syncope. This proneness continues for a relatively long time in consequence of the slow oncoming of depression of the vagus inhibitory mechanism. Such syncope is more usually recovered from than that of chloroform for reasons already given. Recovery, however, may not follow such syncope, when it occurs in cases in which the heart was previously pathologically depressed. By means of the apparatus usually employed in the administration of ethyl chloride, concentrations of vapour considerably in excess of 10 per cent. are invariably administered. It is certainly courting syncope to continue such administration for a moment after the abolition of the corneal reflex. If it be desired to continue the administration for a longer period, it is advisable to employ some regulating apparatus whereby a low percentage may be obtained for the continuance of the anæsthesia. What has been said of the pharmacodynamic effects of chloroform applies to ethyl chloride, but to a less degree in all other respects except that of vagus inhibition. What also has been said about treatment of threatened fatalities and about prophylaxis in chloroform administration applies equally to ethyl chloride.

COMBINED SECTIONS: MEDICINE—SURGERY—
ANÆSTHETICS.

DISCUSSION.

DR. J. W. SPRINGTHORPE (Melbourne) advocated what he calls cortical anæsthesia in minor operations, and brought forward the method used in dental operations by dentists, in which ethyl chloride or somnoform is given with a paper-bag balloon attached to an ordinary mask. At the Melbourne Dental Hospital, this method has been used in 10,000 cases with no fatality.

DR. HORNABROOK (Melbourne) said that Barker reported no ill after-effects, but his house-surgeon said there was nearly always headache. Sometimes cases of spinal anæsthesia went well, but by no means always. He tried it in a case of pyloric cancer; collapse occurs, with a low pulse-rate, pallor, and sometimes the patient dies. In Adelaide, there have been two fatal cases. Probably there would be less headache if the patients were very carefully removed from the table, and the head kept elevated.

DR. RAMSAY (Launceston) used local analgesia extensively. He did not like Barker's solution, as one has to wait too long for its effects. He used a solution of beta-eucaine lactate, 1 gram; sodium chloride, 0.6 grams: solution of adrenalin chloride (1 in 1,000) 1 or 2 cc.; distilled water to 100 cc. He had used this with success for appendicitis, ovariectomy, and other major operations.

DR. W. MOORE (Melbourne) believed it could be used much more extensively than it has been. He had at one time hoped that spinal anæsthesia would prove safe and free from ill effects, but his hopes had not been fully realized. He thought that spinal anæsthesia was not going to be adopted generally. Sometimes we fail to get into the spinal canal, sometimes the anæsthesia is not satisfactory, and he must confess the unpleasant effects were worse than he had hoped. But there were many good effects. In rectal and bladder surgery, it is wonderful, from the operator's point of view. He thought that, at the present time, spinal anæsthesia should only be attempted by the specialist.

DR. G. A. SYME (Melbourne) had had many of Dr. Zwar's bad results under his observation, but still he considered that spinal anæsthesia had a considerable field of use. The objections are its unreliability, as well as its bad after-effects. It is sometimes difficult to determine whether the after-effects are due to the stovaine or to the operation. He was using local anæsthesia more and more, but one of its drawbacks is the risk of producing local gangrene; this may possibly be due to using too strong solutions of adrenalin.

DR. T. P. DUNHILL (Melbourne) used local anæsthesia extensively in operations on the thyroid gland in exophthalmic goitre. In many of these cases, with dilated hearts and œdema of legs, general anæsthesia is out of the question, and he finds they stand operation well under local anæsthesia. There is, however, some pain when lifting the goitre up, and a pressure feeling, as if the whole weight of the body were pressing on the trachea. He uses Barker's eucaine-adrenalin solution. The subcutaneous tissues in front of the goitre are infiltrated thoroughly. On the side from which the lobe is to be removed about 10 cc. are injected, under the deep fascia behind and beneath the sternomastoid muscle. To do this, a deep stab puncture is made behind the sternomastoid, at the

junction of the upper fourth with the lower three-fourths. Through this stab, one of Barker's long flexible blunt-pointed needles is inserted, and the solution gradually injected. After the completion of the injection, half-an-hour is allowed to elapse before beginning the operation. He had used this method 70 times for the operation of partial Thyroidectomy, and had never found it necessary to resort to general anæsthesia. One great advantage is that, if one does get acute thyroidism after the operation, one can at once saturate the patients with fluid, by making them drink large quantities, and this is the only satisfactory treatment of acute thyroidism.

DRS. SPRINGTHORPE, ANDREW, O'NEIL, FIASCHI (JUNIOR), and E. SANDFORD JACKSON also took part in the discussion.

DR. ZWAR said he had not noticed less headache after most carefully removing the patient and raising the head.

DR. TATE SUTHERLAND could not allow the statement to go out that patients died after ether in their beds. He had seen no death from ether, *per se*, and if care were taken to keep the patient warm, and to use clean inhalers and paper-bags, no after troubles would arise.

PERITONITIS.

PAPERS AND DISCUSSION IN THE SECTIONS OF SURGERY AND OF OBSTETRICS AND GYNÆCOLOGY.

TUESDAY, 20TH OCTOBER, 1908, 2 P.M.

MR. E. T. THRING, F.R.C.S., President of the Section of Obstetrics and Gynæcology, in the Chair.

ON THE SURGICAL TREATMENT OF SEPTIC PERITONITIS.

ROBERT B. DUNCAN, M.D., F.R.C.S.E., Hon. Surgeon, Kyneton Hospital.

The subject of the surgical treatment of septic peritonitis does not lend itself as readily to treatment as might be supposed. This arises in a great measure, not only from the many problems that present themselves for solution, as from the urgency with which they have to be dealt. Some years since the matter was simple enough, as the disease was regarded as hopeless, and the patient practically left to die. Our duties and responsibilities in this connexion have now largely increased, and in perhaps in no branch of surgery are we more justifiably proud of the results achieved than in this. Thanks to the investigations of Dudgeon and Sargent, our knowledge of the bacteriology of the septic affections of the peritoneum is now fairly exact. That, and a better understanding of its causation generally, has been of substantial help. The symptoms generally are not likely to be misunderstood—the distended, tense, and tympanitic abdomen, the hippocratic face, the running pulse, the continued and purposeless

vomiting, and obstinate constipation are easily discernible and unmistakable. We want, however, on all occasions wherever possible, to anticipate a condition where the symptoms become so defined. It is safe to say that in all cases of peritonitis of a septic nature not a moment should be lost in surgical interference. At the outset, it is as well to consider the question of anæsthesia in connection with such cases. In early cases this question may be a matter of no concern, but in advanced cases it is a matter of grave moment. Such patients are extremely intolerant of the depressing action of chloroform, and which is often sufficient to turn the issue to a fatal termination, of that there can be no reasonable doubt whatever. What, then, is best to be done? At this period there will be no question of diagnosis arising, and, in my opinion, it will be best to give a hypodermic injection of a quarter of a grain of morphia, and follow this up by the administration of ether. We have now spinal anæsthesia at our command, and also local anæsthesia by infiltration. Barker, of University College Hospital, London, has achieved striking results in such cases, where a general anæsthetic would certainly have killed the patient. Another important matter is washing out the stomach, a procedure that should never be omitted when it can be put into practice. The selection of a suitable anæsthetic, washing out of the stomach, and rapid operating are most efficient aids in dealing with this condition. It will materially aid us if we remember that the peritoneal cavity is a huge lymph sac, and contains many recesses in which fluid and inflammatory products can collect. These are mere potential spaces in health, and their sites too well known to require enumeration, but they have a very special significance in the treatment of septic conditions of the peritoneum. Whether the disease is limited or generalising is immaterial, so long as fluid is present the proximity of such pools or spaces must be borne in mind, and their site examined. The causation of septic peritonitis naturally falls into a few fairly well-defined groups. Those connected with the intestine itself are at once the most natural, as they are the most important. Affections of viscera outside the alimentary canal, and pneumococcal peritonitis, which, although not associated with any structure as regards causation, require to be included and kept in view. A case of sudden perforation of an ulcer of the stomach will furnish an admirable example of the septic peritonitis from perforations in general. In acute perforations, which these cases represent, it is to be remembered that the primary shock is considerable, may be so great as to delay the operation, and, in any case, will have to be kept in mind in estimating the patient's condition. To take a moderately severe case, with considerable extravasation of the stomach contents, a rapid septic generalising peritonitis, and all its associated conditions. We are not concerned with the cause, which will be remedied in the usual way. The removal of the exuded fluid, of whatever kind, will be the first pressing indication, and here, at the very outset, we are met with sharp differences of opinion. Some surgeons would in this case practise thorough irrigation with hot saline fluid, others would rely on mopping, whilst others, again, would only permit the most limited interference. I would certainly prefer irrigation, combined with mopping, as the most likely method to effect the desired result. How is it carried out? In addition to the original incision, a fairly large one is made immediately over the pubes, and two large drainage tubes inserted leading to the pelvis. For irrigation purposes, nothing is better than a stomach tube such as is used for washing out the stomach. With this the fluid flows at a fairly low pressure, and at once eliminates a rather serious

drawback, the forcing in of the irrigating fluid at too high a pressure. The irrigating fluid should be sterile salt solution, a teaspoonful to the pint, and at a temperature of about 105 F. The upper abdomen, at the seat of the rent, is first dealt with, the two kidney pockets, and especially downwards towards the right and left flank. The hand will be gently insinuated, at times carrying the nozzle with it, and moved about amongst the bowels, till a very thorough washing-out has been attained. It is unnecessary to specify the amount of fluid to be used, that will depend on the state of the patient and the object to be attained. When it is finished, the tubes above the pubes are removed, the pelvis thoroughly mopped out, and the tubes replaced. How to deal with the distended and possibly parietic bowels is a problem that must be decided and carried out. With whatever part of the abdomen we may be dealing, and whichever of the many causes of peritonitis may be present, this is an imperative duty. The condition of the bowel here is in much the same state as if it had been obstructed. True, its circulation has not been much interfered with, but it may be just as distended and useless. Note the obstinate constipation in a well developed case of peritonitis. The removal of the cause will not immediately help its functions, if at all, and that, perhaps, when it is too late. How is it to be helped in such a way that the operation is not unduly prolonged? In my opinion, by rapid multiple punctures immediately closed with a round needle and silk, or an opening made in one of the coils, and a Paul's tube tied in. If a saline aperient can be introduced, the more concentrated the better, it will be found of much advantage. Other plans of diminishing the distended intestine consume too much time where every moment is precious. The wound in the epigastric region should then be partly closed, a large drainage tube being inserted at its lower angle. But this and the suprapubic drainage would not be nearly sufficient. No drainage is here sufficient without complete drainage of the right and left kidney pouches, slightly below and anterior to the costal margins. Great differences of opinion exist as to the best kind of drain. The best drainage is accomplished by large rubber tubes, filled with gauze, cigarette fashion, or, better still, filled with gauze, leaving the rubber intact. Such drains being in position, it is merely a question of withdrawing the gauze and again filling the tube. As this can be easily and constantly carried out, such a form of drainage seems to me almost perfect. In the results of perforating intestinal ulcers of any kind, we can find the most typical examples of acute septic, spreading, or generalising, peritonitis, and the above is a rough outline of the procedures we would have at once to carry out. This spreading form constitutes a distinct class by itself, as contrasted with a more local affection. By the latter, I do not mean a septic peritonitis of such a distinctly local character as would produce a well-defined abscess, as may constantly be found in connexion with many forms of appendicitis. The condition we meet with is local certainly, but is still active, slowly spreading it may be, and may in time become generalised. In the one we have abundance of liquid inflammatory products, and in the other the conspicuous feature is matting of the intestines and the production of excessive quantities of organized lymph. Here the dictum of a great authority still remains good—"Doing no more than is necessary, or as little as is obvious." Careful mopping, then, will be the chief indication here, with as little disturbance as possible of the matted intestines. True, they may hide pockets of pus, but better to let such remain and become, perhaps, permanently encapsuled, than to begin opening them up when the patient

may not be in a state to bear it, and the process may become interminable. Drainage here, whilst not so urgent as in the graver case, will be both necessary and valuable, as the amount of fluid accumulation is in such cases often very considerable. Referring for a moment to drainage, it is better to do too much than too little. Not only the usual sites, epigastric, suprapubic, anterior and posterior lumbar, and abdomino-vaginal may be chosen, but our practice should be to put a drain or drains anywhere they are needed. I have expressed a preference for large tubes filled with gauze, and I think this is the best. Gauze itself should certainly not be used. It almost immediately contracts adhesions with the structures with which it comes in contact, is difficult and painful to remove, and, when removed, fresh gauze can only with great difficulty be introduced. The question of drainage generally in regard to site and other circumstances is such a wide one that details can only be considered with each individual case. I have tried to indicate the principles chiefly that ought to guide us. But it is a question that must be asked in all seriousness, are these various measures absolutely necessary and desirable in a patient who may be nearly moribund? Would our responsibilities be satisfied, for instance, with simple but expeditious incisions, and the rapid insertion of drains, after removal of the causative influence and attention to the distended bowels, if need be? The weight and influence of those who adopt this plan are not to be lightly disregarded, and the almost phenomenal success achieved is certainly surprising. As far back as eleven years ago, Van Arsdale reported ten cases of generalised septic peritonitis, treated by drainage alone, and eight recovered, a result that would be hard to equal by any procedure. The same publication, the *Annals of Surgery*, relates Murphy's experience two years ago, in which he lost one case out of 29 by simple incision and drainage. These results are not only great, they are epoch-making. How are they achieved? My own impression is that the result is attained by the very simplicity of the measures and the celerity with which they are carried out. Do operators who give chloroform irrigate and swab, and introduce drainage tubes in the orthodox way, take too much time, unduly depress the patient, and hasten a fatal result? In an ordinary critical case, we can gather scarcely any accurate idea of the virulence of the infection or the amount of reserve power available. It may well be that in such cases our procedures are too protracted, and that as the influence of the results of simple incision and drainage become more pronounced, it will influence our treatment powerfully in that direction. Time in such cases seems to be a most precious element of safety, and the continuous recognition of this fact will no doubt benefit our results. The immediate after-treatment by posture is an almost self-evident requirement to follow what has been done. The patient, being returned to bed, should have the head of the bed raised at least from 12 to 18 inches. This has only one serious drawback, that the position is not suitable for the lowered blood pressure or distinct shock that often obtains. The value of this posture in providing natural drainage from the upper to the lower abdomen is incalculable, and no factor has been more conducive to success than this. But why should the presence of septic material be more harmful in the upper than in the lower abdomen? The toxic absorption in the upper region is much greater and quicker than in the lower. Here the lymph channels are not only numerous, but large, and continuously admit large quantities of toxins into the circulation. In the lower abdomen, and in the region of the pelvis, on the other hand, the lymph channels and absorbents generally are not only much smaller,

but are early shut off by the inflammatory process. The presence of the inflammatory material and toxins when they are conveyed to this region are comparatively harmless, and one great source of danger is to a large extent neutralized. The trend of fluid and particles also is always towards the diaphragm unless corrected by this postural position. A necessary adjunct of the Fowler posture is the continuous introduction of saline fluid into the system by way of the rectum. This has several obvious and conspicuous effects. The reversal of the flow in the peritoneal lymphatics will at once have a salutary effect on the absorption of toxins. In the blood itself the toxæmia will be largely neutralized, and a freer excretion by the kidneys promoted. Care must be taken that the flow is slow but continuous, and this admits of easy regulation. Words are almost inadequate to express the influence of these two procedures on the disease under discussion. Much as we have attained in the treatment of this formerly hopeless disease, it is a question if we will not at a very early date very considerably modify our present methods in favour of the American operator, to whom we are indebted for the saline rectal treatment. I cannot help remembering occasions where flushing, mopping, and other time-consuming operations were followed, that the patients' chances were simply extinguished by the delay. I am sure it would be well in critical cases, which most of these are, to bear in mind what has been achieved by light or local anæsthesia, rapid operating incision, and drainage, and the avoidance of even suturing, to accomplish the minimal amount of work necessary in the quickest possible time that we are individually capable of doing.

SEPTIC PERITONITIS: ITS SURGERY.

WM. CHISHOLM, M.D., Lond.,
Surgeon to the Sydney Hospital.

When Molière, who delighted in lampooning the pretensions of the medical practitioners of his day, wrote one of his often-quoted passages, he little thought that the words put into the mouth of one of his characters might one day be taken to accurately describe a radical change, on the part of his more scientific successors, in the treatment of one of the most fatal diseases which befall mankind. When we consider the change in the treatment of peritonitis from opium and its euthanasia to that of active surgical interference, we may say, but with more honesty than did Sgaranelle, "That was formerly so, but we have changed all that, and practise now in quite a new manner."*

It is hardly too dogmatic to say that there is, at present, no medical treatment for septic peritonitis, and the title of this paper seems to imply that the treatment of this disease has now passed definitely into the domain of the surgeon. I would I could prove more worthy of the high compliment which has been paid me in asking me to write upon this subject. In doing my best, let me at the outset renounce any claim to originality; the methods I advocate are such as have appealed to me as the most satisfactory, and are founded on the results of hospital and private practice, wherein I have endeavoured to assimilate with my own ideas what has seemed best of the various improvements introduced by different surgeons

* *Le Médecin Malgré Lui*, Acte II. Scène vi.—*Géronte*. On ne peut pas mieux raisonner, sans doute il n'y a qu'une seule chose qui m'a choque : c'est l'endroit du foie et du cœur. Il me semble que vous les placez autrement qu'ils ne sont : que le cœur est du côté gauche, et le foie du côté droit.—*Sganarelle*. Oui, cela était autrefois ainsi : mais nous avons changé tout cela, et nous faisons maintenant la médecine d'une méthode toute nouvelle.

from time to time. The change in our treatment of septic peritonitis is, like all other advances in our art, founded on a truer conception of the pathology of the disease. It is not within the scope of this paper to discuss the pathology of peritonitis, nor to go into the question as to how far the phenomena of inflammation are to be regarded as entirely vicious and how far as protective influences which make for repair (1). Corner (2) says bacteriology gives but an imperfect account of the various lesions in peritonitis; but that we must look to pyogenic micrococci for the starting point of cases of acute perforation and gangrene in whatever part of the alimentary tract it may be situated, and while there is ample evidence that the pyogenic cocci are the great causes of acute infective necrosis in any part of the intestinal tract, they are liable in the subsequent course of the disease to be killed out by the growth of the *bacillus coli communis*, and to be entirely absent from the cultures. Seeing that the *bacillus coli communis* has been found in the peritoneal cavity in cases of intestinal obstruction and in the sacs of strangulated herniæ without producing any obvious result, it is, on the one hand, argued that it is not clear that, unaided, the ordinary colon bacillus of our intestine does us more harm than good. On the other hand, C. B. Lockwood (3) states that he has learnt to look upon the colon bacillus as one of the most dreadful scourges of the human body. Perhaps a reconciliation of these conflicting opinions may be found in Corner's admission that it is by no means easy to make sure of the ordinary *bacillus coli communis*. Be this as it may, as practical surgeons, we shall do well to act on the assumption that the various fluids found in the abdominal cavity in cases of peritonitis contain the products of the activity of various micro-organisms, and that it is our duty to rid our patients as far as we can of these highly toxic products. At the present time this can best be done by opening the abdomen.

Peritonitis commonly follows on some lesion, often more or less sudden, involving one or more of the viscera. Perforation of a gastric or duodenal ulcer, rupture of the gall-bladder or of an hepatic abscess, are the commonest starting points in the upper abdomen. Lower, we meet with the peritonitis associated with disease of the appendix, with the necrosis of bowel due to internal strangulation, or with the perforation of a typhoid ulcer, while in the pelvis we have as starting point an infected fallopian tube, or a pyosalpinx, generally gonorrhœal, a suppurating or twisted ovarian cyst and the milder forms of peritonitis associated with a ruptured tubal pregnancy. Then we may have peritonitis due to penetrating or gunshot wounds in any part of the peritoneal cavity, and various forms of diffuse peritonitis due to streptococcal and pneumococcal infection.

Briefly our surgical treatment aims at—

1. Dealing with the source of origin of the peritonitis.
2. Removing, as far as possible, the extravasated contents of ruptured viscera, together with any effused fluid which probably contains the products of bacterial activity the absorption of which acts on the patient as a most virulent poison.
3. Drainage by means of rubber tubes containing gauze for about the first twenty-four hours to remove all remaining toxic material, or packing with gauze for a longer time to shut off localized abscess cavities from the rest of the abdominal cavity.
4. Incising and emptying as far as possible distended and paralyzed intestines.

It is of the first importance in dealing with a case of peritonitis to ascertain the cause, and deal with this in the first instance. In the large majority of cases a median vertical incision is the most satisfactory. When a ruptured gastric ulcer is suspected the incision may at first be above the umbilicus, but it may be necessary later for purposes of cleansing and drainage to make incisions in the loin or above the pubes. The escape of odourless gas or the presence of lymph or particles of food mixed with more or less turbid fluid would direct our attention to the stomach; if bile is present we look to the gall-bladder or duodenum. If there is an offensive odour of the fluid present we are guided rather to the appendix. Cases originating in the pelvis are most frequently localized. In cases of difficulty in ascertaining the origin of the peritonitis, help is often gained by observing patches of lymph; this protective action of the peritoneum will often guide us to the source of trouble. One cannot, within the limits of this paper, discuss the treatment of the various lesions which may be met with; they must each have their appropriate surgical treatment. In these days of general anæsthesia there is too much leisurely operating; speed should be cultivated, and no rough handling of the parts, whereby the delicate epithelium of the intestines is further damaged, should be allowed: both these points are of vital importance to the patient. No one line of treatment can be laid down for all cases; the surgeon must bear in mind certain principles and apply them with judgment to the circumstances of each individual case, being careful to keep well within the limits of toleration of his patient. This point is insisted upon by Binnie, who aptly says, with reference to closing the abdominal incision, "The inexperienced surgeon, after completing a prolonged operation on an exhausted individual, sometimes forgets that it is better to have a post-operation hernia in a living patient than a perfectly closed wound in a corpse."(4)

As generally described in connexion with appendicitis, we may consider three forms of peritonitis as commonly met with.

First.—When the peritonitis is localised, the focus of origin and any extravasated fluid or pus being securely walled off from the rest of the peritoneal cavity.

Second.—Where there is little or no attempt in thus limiting the inflammation which is spreading more or less widely from its seat of origin, but where the greater part of the abdominal cavity is still healthy.

Third.—A condition of more or less general peritonitis, where the inflammation has spread over the small intestines and into the loins, and the pelvis contains turbid, purulent, and often offensive fluid, which may extend more or less throughout the whole abdominal cavity.

The first class is seen perhaps most commonly in appendicitis, but also in connexion with ulceration, simple or malignant, of the stomach and intestines. These cases are treated by making another incision if necessary over the most suitable spot, and by wiping the cavity clean; for this purpose I still prefer soft pieces of carefully prepared marine sponge. The cavity may be dusted with iodoform and then either drained with a tube or packed with gauze as seems best to the operator. The second class is seen also in some cases of appendicitis and in rupture of stomach or duodenal ulcers. In the majority of these, a similar line of treatment can be followed; some of them, especially those connected with lesions

of the stomach, may be safely treated without drainage, but my own preference is to drain in all, if only with a small piece of split rubber tube containing gauze. When the escaped contents of the stomach or duodenum have passed down into the right iliac fossa or pelvis, it is wise to irrigate in many cases and to drain in all. The Fowler position should be adopted in the after treatment of these cases.

It is in the treatment of the third class, that of more or less general peritonitis, that the greatest diversity, both of opinion and practice, is met with amongst us. It is only natural that a surgeon should cling to such methods as have given him good results in spite of the fact that other surgeons claim equally good results from opposite methods. Our own successes speak to us in flattering terms, but we should remember that the experience of the greatest amongst us is limited, and when priding ourselves on a fortunate result in some difficult case, owing doubtless to the excellence of our own special technique, it would not be amiss to recall the modest words of Ambroise Paré, "I dressed his wound, and God healed him." My own experience is in favour of irrigation with warm normal saline solution and subsequent drainage. Mr. C. J. Bond (5) states that in infections of great virulence such as that due to the bacillus coli communis in appendicitis and intestinal necrosis, irrigation of the general peritoneal cavity is harmful in removing protective phagocytes and spreading infection. While having the greatest admiration for Mr. Bond and his work, more especially as an old fellow student, I am inclined to regard this view rather in the light of a sentimental tribute to the conservative processes of nature than as a guide to sound surgery. At the risk of dislodging some friendly phagocytes, it is best to wash out the foul fluid with its deadly toxins. By irrigation is not meant filling the abdominal cavity as full as possible with saline solution, and then letting it escape through the same incision as best it may. This will probably only serve to spread infective material still further throughout the abdominal cavity. In cases requiring irrigation, it will be necessary to make incisions in one or both loins as well as in the middle line above the pubes. By raising the head and shoulders of the patient, and directing, under low pressure, a stream of warm saline solution from above downwards, from the less infected to the more infected part of the abdominal cavity, while at the same time the lower wound is held widely open, the irrigation can be efficiently and safely carried out. This may be further aided by a large tube being passed from above the pubes deeply into the pelvis. No limit is placed to the amount used, but it should be continued, if the patient's condition is holding out, till the fluid escapes clear. Most of what remains can now be removed by the careful use of sponges on holders. In these cases, also, I strongly advocate drainage by means of split rubber tubes containing gauze passed deeply into the pelvis from above the pubes, and also in the loin incisions. In the female, drainage through Douglas' pouch into the vagina is of the greatest value. We are all now familiar with the fact that drainage is useless after some hours, and that the track where the tube is, is soon shut off from the rest of the abdominal cavity, and that even the capillary action of the gauze is destroyed by the coagulation of lymph in its meshes. But in these few hours much fluid, which is in all probability highly toxic, is withdrawn from the abdominal cavity instead of remaining to be absorbed by the patient. It is my practice to order fresh gauze to be placed over the ends of the tubes every hour or two for the first six hours, by which time the drains will have done their best work. They can generally be removed in 24 hours, but gauze, if

packed into an abscess cavity, had better be left for a week or more until it comes away easily. The portion of peritoneum covering the under surface of the diaphragm is the most active area of absorption. It should be our endeavour, unless the necessity for dealing with the causative lesion compels us to do so, to interfere with this area as little as possible, and so to carry out our irrigation as to avoid conveying infective material to this part.

The question of whether recovery is possible from general peritonitis, *i.e.*, peritonitis in which every part of the peritoneum is involved, is rather of academic than practical interest. I should think that by the time the whole peritoneum is involved in a septic process, as distinct from tuberculosis, the question of operation would hardly arise.

The necessity for incising and emptying distended coils of intestine in cases of intestinal obstruction was, I believe, first insisted upon by the late Mr. Greig Smith, as being only second in importance to the relief of the obstruction. In septic peritonitis, the same importance is attached to emptying the distended and parietic bowel. The contents of the bowel seem a favorable culture medium for most virulent micro-organisms, their toxins are absorbed, thus still further prostrating the patient, and the organisms themselves can escape through the thinned and inflamed bowel wall. The method advised by Moynihan (6) of incising the bowel and introducing a glass tube, enables this to be done satisfactorily with little danger of allowing the contents to escape into the peritoneal cavity. I remember a most instructive illustration of the benefit derived from merely opening and draining the bowel. Many years ago I was called up early one morning to a case of peritonitis at the Sydney Hospital. On opening the abdomen in the middle line, distended coils of small intestine presented; the man's condition seemed too bad to permit a thorough investigation; I stitched with difficulty a small area of distended bowel to the parietal peritoneum, and packed the wound with iodoform gauze. In a few hours I returned to the hospital, the patient's forehead was then beaded with perspiration, and his condition looked hopeless. The presenting bowel was at once punctured, allowing the escape of its contents. I never saw anything more remarkable than the change in that man's expression within a quarter of an hour. He went on quite satisfactorily without further treatment, and the incision healed readily. I heard a few years after that he died of some lung affection, so this was probably a case of tubercular peritonitis. In another case, after washing out the abdominal cavity, the almost purple intestine was incised and drained; owing to the alarm of the anæsthetist, and our desire to get the patient off the table alive, the incision was sewn up with one layer of Lembert's suture only, and the median incision was closed most imperfectly by a few through and through silkworm gut sutures. To our immense surprise, the patient recovered. A second operation was necessary for the better closure of the abdominal wound; but before this had thoroughly healed, the patient, a regular "Warrigal," slipped away from the hospital. He afterwards served in the South African war, and I saw him some months ago. The abdominal wall was sound.

In the after-treatment, the Fowler position, or propping the patient up in bed in as nearly as possible a sitting position, is now very generally adopted; by this means all fluid is drained away from the absorbing diaphragmatic peritoneum already referred to, into the pelvis, whence it can escape by the drainage tubes. Should pus subsequently form, this is more easily dealt with in the pelvis than under the diaphragm. When these

late pelvic abscesses tend to point in the rectum, I am strongly in favour of opening them per rectum instead of attempting to reach them through the abdominal wall. Of the method of continuous saline rectal injection introduced by Murphy, I am unable to speak from my own experience. Carwardine (7) refers to it as one of the most valuable suggestions of recent times, and many others speak almost equally in its favour. Fleet-Surgeon J. Falconer Hall, tells me of the remarkable results he has seen in the treatment of cholera by this method. It is conceivable that when the vital processes are at a low ebb, absorption of fluid from the rectum would be aided by the increased pressure—a literal *vis a tergo*—of the fluid administered in this way. But I hope I may say, without offence, that as there are always some amongst us eager to take up *le dernier cri*, whether it be the latest coal tar product, a new aseptic ritual, or some modification of surgical technique, so this method has been used, of course with success, in many cases which would have got on quite well without it. In very bad cases it is often necessary to administer a pint or more of saline solution sub-cutaneously, while the patient is on the operating table. In most cases, I order from 10 to 20 ounces of saline, sometimes with an ounce of brandy, to be administered per rectum every two, three, or four hours after the patient is back in bed. This is persisted in according to the patient's condition and ability to retain it. There is certainly no risk of the patient absorbing more than he needs; but the question is, does he in this way absorb as much as his system requires, and, in desperate cases, whether this end may not really be better gained by Murphy's method?

In the after-treatment, all the usual aids, such as warmth, hypodermic injections of strychnia, fresh air, &c., have to be brought to our assistance. Opiates, as a rule, should be avoided, but in cases of great pain or restlessness a small dose, gr. $\frac{1}{20}$ to gr. $\frac{1}{6}$ of heroin may sometimes be given hypodermically. Thirst may be complained of. If the saline injections do not relieve this, water preferably hot, may be given by the mouth as desired. I think it cruel to withhold water, even if the patient vomits after it, this probably has a beneficial effect in washing out the stomach. Some surgeons employ gastric lavage by means of a tube; the only objection to this is that it may unduly distress the patient, or be resented by him.

If the patient's appearance and general condition keep good, I prefer to interfere as little as possible for the first 24 hours. At the slightest sign of abdominal distension, especially about the epigastrium, turpentine enemata, as hot as can be borne by the patient, are given every few hours, until a satisfactory action is obtained. Failing this, calomel in gr. i. doses hourly, may be given for five or even ten doses, and followed by salines if necessary. Little food beyond albumen water or white wine whey need be given for the first two or three days. Nutrient enemata are chiefly useful by reason of the fluid they contain. If the abdomen remains soft and free from distension, the tongue cleans and becomes moist, vomiting ceases, the bowels act and the pulse keeps below 100, while the temperature shows little variation from the normal, the patient will probably do well. On the other hand, a distended abdomen, dry tongue, vomiting, a failure to get the bowels to act, a rising pulse with perhaps a falling temperature, afford a bad prognosis.

As to statistics, I have none to give. "For which relief," I fancy you will say, "much thanks." While admitting that amongst a very large number of patients there will probably be a fair average

of the severer types of the disease in each successive series of 100 cases, and therefore it may be thought possible to draw a fair inference as to the merits of different lines of treatment at the hands of any one surgeon, I regard the possible fallacies as so many that I cannot work up any enthusiasm on the subject. The increased experience and skill of the surgeon will cause his later cases to make a better show than his earlier ones.

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DISCUSSION ON PERITONITIS.

DR. HINDER (Sydney) said:—Some years ago, in my first case, I came to the conclusion that the best thing was to wash out the abdomen with saline solution. I carried out that practice, and I do not think my results are one whit the better now than five, six, or seven years ago. I wish to draw particular attention to the Fowler's position and the introduction of saline solution by the rectum. I have adopted both. Perhaps my opinion may differ from a good many other surgeons. I may say that I have not obtained any better results because I used that treatment. I used it for a short time, but not for a great length of time, because it did not appear to me to be as serviceable as we imagined. In regard to the Fowler position, I have found it unfavorable if the patient is very ill. With regard to the rectal infusion, I have found that a very small percentage of patients will allow you to introduce large quantities of fluid into the rectum and retain it. The nurses would introduce pint after pint, but a large proportion of it escaped into the bed. Other patients would absolutely refuse to have the tube continuously in use. I have adopted the method of supplying about half-a-pint or a quarter-of-a-pint for every half-hour. My own impression is that the reason why our results are better now than they used to be is generally that the general practitioners have been taught to recognise peritonitis much more rapidly, and we come across cases much earlier. As to the treatment of introducing a tube and draining the patients without washing, I cannot help but think that the man who makes a small opening and puts the tube in the opening and drains, and nothing else, does not really know to what extent the peritonitis has gone on. I think the man who spoke of patients suffering from general peritonitis makes this mistake. The fluid higher up, for example in the kidney pouch, may contain no bacteria.

DR. FIASCHI (Sydney).—The question of washing out the infective fluid in general peritonitis has been raised by both speakers. Dr. Chisholm is in favour. Dr. Duncan is not quite sure about it. Without giving you any scientific demonstration, I will tell you what I have done for a long

time, and I am pleased with the result. I do not wash out the peritoneum, as Dr. Duncan says, with a stomach tube, but with a Blake's tube, which I have brought with me to show you. With this tube you can wash out the peritoneal cavity in three minutes. I do not find that there is any time lost. Dr. Hinder mentioned that he did not believe that true acute septic peritonitis could have any recovery. I beg to differ from him. I think, if you are quick and wash out the peritoneum thoroughly, you give your patient a chance. Dr. Blake, who introduced the tube in New York, has reported 70 cases of washing out with the tube, with fifteen deaths only. That is a big record.

DR. BOWKER (Sydney).—I agree with Dr. Duncan in doing the greatest amount of work in the quickest possible time. With regard to the injection through the rectum, I agree with what Dr. Hinder said, that there are very few patients who will tolerate the constant and continuous irritation of the rectum. There is not the slightest doubt that the solution of saline used intermittently can do good. In my own practice, whenever I get a case of peritonitis, or, in fact, any abdominal case whatever, I give before operation a dose of calomel. Then, with regard to opening the bowel, I think the bowel is too often opened by the surgeon. If the bowel is distended, there seems to be a tendency to open small intestines, with the result that papers have been read on the best means of closing up this opening. I think that frequently the small intestines are opened when ordinary medical means would overcome the trouble. In regard to washing out, some years ago I used it invariably, and very thoroughly, and although I do not know that I did it as Dr. Fiaschi does in three minutes with his tube, I did it pretty effectually, and it did not take very much longer. Since then, I have found by my own experience that those cases did well which were not washed out. The researches of Dudgeon and Sargent point out how this result occurs. So, on the whole, I think it is much wiser to swab out, and not waste the three minutes Dr. Fiaschi refers to.

DR. MAITLAND (Sydney).—There are just two points at issue in these papers. The first is the question of drainage, and the second that of washing out. My own practice has been in a localised peritonitis to simply drain and remove the cause, if possible. In peritonitis which is somewhat diffused, drain and do not wash out, but in peritonitis which is practically general, wash out and drain. I am quite sure that the main essential is drainage. I frequently used to wash out more than I do now, and I grant, with Dr. Hinder, that I do not think it makes very much difference whether you wash out or do not. I think Dr. Murphy's paper on general peritonitis was one of the best papers I ever read. It is based on sound common-sense.

DR. WORRALL (Sydney).—Even so far as the debate has gone, it must have struck every visitor that a great variety of technique has been upheld. No previous speaker has touched on what I consider the two chief points leading to good results in these cases. First, early operation; and, second, removal of the focus. I would like to urge on the younger members of the profession that when they come across a case they should never leave that case more than two hours until they have made up their minds whether they will operate or not. It is ignoring that rule which leads to deplorable results. The practitioner must not be deceived by the onset of apparent recovery. I say that early operation is the chief factor in obtaining good results in septic peritonitis. In regard to puerperal septic peritonitis, I should like to advocate Pryor's operation, which I have had a very large

experience in, and that is emptying Douglas' pouch of the serum which it contains, inserting a gauze, which excites a fresh flow of serum to the affected parts, and thus giving the patient an opportunity of overcoming the disease. The best drainage, in my opinion, is a tube containing a strip of gauze, which shall not penetrate beyond the end of the tube. I think Dr. Duncan is very wise when he says that this was the best form of drainage, because the gauze could be removed from the tube and a fresh piece of gauze could be inserted. If you put in gauze alone, without the rubber, you insert something which is good for shutting in poisonous stuff, but is absolutely useless as a drainage. One other point in regard to desperate cases. I believe doses of morphia are indicated. I had one case which was so bad that I never troubled to see how it was. I thought I would see what the *post-mortem* disclosed. I saw the patient, and found him better. That is a case in which I had given half a grain of morphia. I do not think that in desperate cases large doses of morphia are always advisable; it is a great mistake to think that the patient should be allowed to continue to suffer because some have said that morphia is always bad. Morphia sometimes does good.

DR. J. A. G. HAMILTON (Adelaide).—I think the strong point about septic peritonitis is early diagnosis and early operation. Some years ago I tried washing out the abdominal cavity in a case of ectopic gestation. If any man will try and wash out an abdomen completely of blood clot, he will be surprised at the amount of time it takes. I maintain, no matter how you wash out, you cannot wash out the septic material thoroughly. It certainly requires a length of time, that is more injurious to the patient than if it was not done. I think the feature of later operation in septic peritonitis has been the time saved by being satisfied with swabbing, rather than by washing. In females, I drain Douglas' pouch—it is lower than suprapubic drainage. I am rather surprised to hear Dr. Hinder and others say they have not had good effects from the Fowler position. All I can say is that, in Adelaide I think the majority of men have come to the conclusion that it is a good idea in the treatment of septic peritonitis. I personally have seen effects which I cannot but put down to a great extent to the position. Dr. Bowker says he does not find that the patient will stand continuous rectal saline, and he prefers half-hourly saline injections. All I can say is that to disturb a patient every half-hour is not a good idea. If the saline into the rectum is given very slowly, it can be retained.

DR. SYME (Melbourne).—I quite agree that one of the causes of better results in the treatment of peritonitis is that cases are operated on earlier. Without going into statistics, I do not know anything that has been more remarkable in my experience of surgery than the extraordinary improvement in the results of treatment of cases of peritonitis within the last few years. Rapid operation—as rapid as possible—consistent with removing the cause, establishing drainage, associated with the Fowler position and the use of saline injections, are very important factors. It is not one thing; it is a combination of things which all come together that has so improved the results; and it is my experience—and I think Dr. Moore will bear me out—that the results at the Melbourne Hospital have been very remarkable. With regard to the use of the saline solution in the rectum: As Dr. Hamilton said, a great deal depends on the way it is done. I find the greatest difficulty in getting nurses to do it properly. You have to supervise it your self. You must see that the flow is going in at the proper rate, and not going into the bed. Of course, if you let it go into the bed, it will not do any good. In those cases where it is retained, you get good

results. There are cases, I admit, where persons will not retain it. Where you can apply it, I am personally satisfied of its great benefit. With regard to the Fowler position: It is said that when the patients are in such a condition of prostration, to prop them up is practically to kill them. It is not so. I find they do stand it. I thought, at first, it would be a dangerous thing to do. But the relief that is given to the heart by allowing every thing to drag down, away from the diaphragm, counteracts the bad effects.

DR. MOORE (Melbourne).—With regard to the statement of Dr. Chisholm that medical treatment was of no avail, and then, later on, what some one said, that a case of general peritonitis never recovered; I think it is a great pity that those of us who are old enough to have seen cases of general peritonitis before operative treatment was thought of should have completely forgotten what must have been some of our experiences. I think, even in regard to operating, some of that experience may be of the very greatest value. Now I am perfectly certain that when I was a student I saw cases of general peritonitis which did recover without surgical treatment, long before surgical treatment was thought of. One case, one of the earliest cases, was in an alcoholic woman. Soon after her death I opened her abdomen, and had the very greatest difficulty in dissecting even the stomach from the anterior abdominal wall. The whole of the organs were in the same condition. Surely that must have been peritonitis general enough, and must have occurred years before in this particular patient. I can scarcely imagine peritonitis more general, and you would not meet with a more severe case, and yet that patient had recovered. Much more recently I have seen a patient suffering from general peritonitis, where we could not at that stage ascertain the cause. The patient was so bad we agreed that it might be possible to get her on to the table, but that was as much as we could do, there was absolutely no hope of getting her off the table. I said I would give it up. On second thoughts, I made an incision and drained the peritoneum, and the patient recovered. That is an important thing to remember where it applies in a surgical case, that in some cases you have a doubt in regard to drainage, and if you feel that a patient might have possibly recovered without your operation, I think that then you may safely omit your drainage. As a rule, when free fluid is found, and the patient recovers after operation, the peritonitis is not general; but in some of these cases that recover, the peritonitis is general. With regard to the question of washing out, the position he would like to take up would be a simple one if right—that you could wash out in every case. A physician used to say that if the temperature in typhoid goes to 101, give 5 grains of quinine; if over 103, you give 10 grains. That made the treatment of typhoid exceedingly simple. If you could see your opportunity to wash out in every case of peritonitis, or not in any case, then the matter would be very simple. I am quite satisfied that the treatment of general peritonitis is not as simple as that. You get different cases, and in many of these I am certain it is not necessary to wash out. Whether you do a great deal of harm by washing out, I am not so sure, but I am sure you should never do in surgery what is not necessary; so in a great many of these cases I do not wash out. These are cases where the fluid is not foetid, and where it is turbid. Then you come to other cases, where the fluid is foetid, where the quantity is great and distinctly putrid in appearance, and in these cases I think it is better to wash out; and then I think the patient might recover with washing. I am equally satisfied recovery in these foetid cases is much more rapid when they are washed

out. I will give you two cases. One was an old woman, dirty and neglected, about 65 years of age, and just about as bad a subject as you could have to operate on. She was so bad I would not ask any one to give a general anæsthetic. This patient had hernia. It was very difficult to be sure of what condition the abdomen was in. We gave her stovaine. I made the incision for the hernia, but found it was full of omentum and pus. I was satisfied at once that the cause was not there. I opened the abdomen in the middle line, and the abdomen was full of pus. I removed the gangrenous appendix at once. I passed the drain tube through the median incision to the bottom of the pelvis, and washing out went on while I dealt with the hernia. By the time I finished the abdomen was clean, and I closed the abdomen, and then we adopted what is my routine treatment at the hospital, by seeing that constant injection was made into the rectum, and the patient made a perfectly good and rapid recovery. Some time afterwards, a girl came into the hospital with abdominal conditions. There were about six reasons why I decided not to operate. The illness came on after she had eaten of crayfish, which she said she knew to be bad; and I regret to say we waited some time. I opened the abdomen and found a quantity of pus there. I removed the appendix and drained. She looked like recovery. This went on for a fortnight, and the only thing troubling us was how we were going to deal with the abdominal wound which had been drained. But she then got bad, and finally she died. There was a case not washed out, and one which wanted it. When the abdomen was opened after death, it was found she had got several collections of pus throughout the abdominal cavity. Had that patient been washed out, it would have been very easy to say that the pus had been carried through the abdomen by the washing out. But she was not washed out. Dr. Syme spoke of the results at the Melbourne Hospital. These have been wonderful in recent years. I do not know whether at any other hospital you can get the experience we do at the Melbourne Hospital. It is the place where everything that is bad and nasty is usually sent. Not very long ago, I had four cases of general peritonitis in five days. In three successive days I had cases of general peritonitis, and not one of these was through appendicitis. One of these cases was exceedingly interesting in regard to the question of the continuous injection in the rectum. That was a case due to pyosalpingitis. She was an elderly woman, very stout and almost moribund, when we operated. She was constantly vomiting. I managed to deal with the local condition, then drained, and used the continuous injection. She went on for four or five days vomiting frequently a dark fluid in large quantities, but fortunately she was retaining the saline. She retained as much as 9 pints of saline in the day, from seven to nine she retained for four or five days. I did not mind the vomiting so long as I knew she was retaining the saline. I kept on, and she made a good recovery. I venture to say that such a case as that some time ago would have been considered absolutely hopeless under any method of treatment we then knew. In both these cases, Fowler's position was used, and the continuous saline. Only the other day, I operated on a man who had a large kidney filled with pus. About 6 pints of fluid were in the distended kidney. A couple of days after, he got those terrible symptoms which we all know. I am certain he would not have had a hope only for Fowler's position and constant injection. This man said he would be easier if we would sit him up. This was at a time when he looked as bad as he could be. The man is making good recovery. With regard to opening the distended intestines: I do not think we open them often

enough. We speak about late operations, and, again, that the patient should be seen to early. I do not know other surgeons' methods. I know we ought to operate earlier. I find myself now and then making a dreadful mistake in letting a patient go on under my eyes all the time. I recently operated on a patient for the removal of an appendix. He had been operated on three times. I had operated on him twice. The last time I did it he nearly died after the operation. After a few days he got distended, and complained of a great deal of pain. I let him go on from the Monday afternoon that the operation was done, and on Saturday he started to vomit freely, and I then thought that there was obstruction, and that he was going to die in a very little while unless something was done. We opened his wound. He was so bad you could do anything you liked to him; and at the bottom of the wound I found he had got septic peritonitis. I got a coil of small intestine, drew it into the wound, and make a small incision, and drained it. I did not get a great deal from that, and was not satisfied. I got another coil, and then I unfortunately found that it was the same coil. I then got a piece of rubber tube, and pushed it about nearly 3 feet along the small intestines, and when I got a certain distance, gas simply gushed through his abdomen, and the man said, "I feel much better." That was one advantage of not having the anæsthetic. I knew I must be pretty close to the cæcum. I went along, and got as close to the cæcum as I could, and made another opening, and passed this tube along about 3 feet, and left it there. I was out of town for three or four days, and on coming back again I did not like to ask how the patient was. I ventured up to see if the bed was empty. I went up, and he was there, and he is making a good recovery. Some time ago I had a similar experience. A patient had been operated on for general septic peritonitis. He had got this distension. I opened the intestines, and that patient recovered. Both these patients, I am satisfied, owe their lives absolutely to the Fowler position and continuous injection.

DR. FIASCHI, Junior (Sydney).—I merely want to say a few words as regards the Murphy injector. I had a considerable experience of this invention during my stay in the New York Hospital when this method of treatment was brought to us. We have used one of these injectors through the rectum with very good results. The main thing is not to pull the tube out of the rectum. When it is once in, you should leave it in. A little cork should be inserted, and the tube left in the rectum. In regard to the elevation of the bag. The bag must not be elevated. It must be just enough to allow a bubble to come to the surface of the water. I think when I came out here I was one of the first to use this method, and, so far as our experience goes, we have never had any trouble. If you leave it to the nurses, you will get into trouble. Unless you watch the process yourselves, you would not, sometimes, recognise the Murphy invention without the label. Just a word about the Fowler position. That position has come to stay. I suppose that you have all noticed that sometimes when the body is elevated it is hard to keep the patient up. When suffering from peritonitis, all the muscles lose their tautness, and patients have a tendency to slide down. One good scheme is the bed-rest devised by Dr. Allenbury, which is a double-inclined plane. The advantage is that the body is flat and the patient is held up by the angle. For those who have not got this apparatus, the ordinary deck-chair is just as good, with a hole out in the bottom. You need not put the patient on the bed, but on the floor.

MR. E. T. THRING (Sydney).—There are one or two things I would like to more or less endorse, and one or two things I would like to emphasise. One is that very little has been said about what the patient does himself in the way of dealing with organisms, and about efforts to promote phagocytosis. I think there is no doubt about it that saline solution in the peritoneal cavity has had a beneficial effect in dealing with peritonitis. At the same time we cannot overlook the fact that early diagnosis has certainly done much during the last few years, and so we have prevented the localised peritonitis from becoming general. All the same, we have to deal sufficiently frequently with general peritonitis. In regard to the question of irrigation. Each case must be judged on its merits. When one is in doubt as to whether to close or drain, I am quite sure we will help the patient by leaving saline solution in the cavity. It helps, not only the viscera, but causes all the intestines to float into the position which they would like to assume, as well as making the patient easier and relieving the thirst. One doctor advocated large doses of morphia. I do not think I would like to treat even a desperate case with large doses of morphia. My personal feeling is to avoid it as much as possible. The net result of the discussion is, I think, that most of us are agreed that we get better results than in the past, and that we do so by early diagnosis and by the judicious use of saline solution one way or the other. The question of the manner of injection we must leave to be discussed on the merits of each individual case.

DR. DUNCAN (Kyneton).—There appears to be a fair unanimity of opinion about the essentials of the case, and with that at the present moment we will have to be satisfied with. Time alone will prove whether certain progressive suggestions made are good or not. In regard to the injection of saline, I would say that it should be given in the most favorable position, and would impress on your minds the necessity of carrying out the directions given by a most eminent American surgeon as to the manner in which it should be done. This is by the drip method, so that the patient will get the full benefit of the absorption.

SECTION OF MEDICINE.

PRESIDENT'S ADDRESS.

THE MENTAL FACTOR IN MEDICAL PRACTICE.

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My first duty is to express to you my hearty thanks and appreciation of the great honour you have conferred upon me in inviting me to preside over the deliberations of the Section of Medicine during this Congress.

In thinking over a subject upon which to address you, I at once put aside any idea of giving you a review of the work accomplished in the field of medicine during the last few years, since it would be impossible

to do justice to such a wide subject, and I came to the conclusion that the President of a Section of the Congress should utilise his opportunity to put forth some work, if possible, of original research, or, at any rate, some record or review of the special line of work in which he may have been occupied. As we advance in years, in knowledge, and in experience, we come to see things in different lights; and with a broader view of the landscape we can take our bearings with greater accuracy and pursue our course with greater confidence.

Consequently I have decided to bring before you a line of thought and work which has occupied my attention, more especially during the last few years, and which has been forced upon me by the special line of practice in which I have been engaged. Briefly, I would put my thesis before you in this way. I believe that, while in a general sort of way most practitioners of medicine recognise the influence of the mind on the body, comparatively few of us have thought out the meaning of the expression or its full significance in medical practice, or realise the necessity of taking this factor fully into consideration in our therapeutic measures.

I consider the present an opportune time for drawing attention more especially to this subject, since—as a result probably of increasing strain on the nervous system imposed by the economic and social conditions of modern times—there seems to have occurred a marked increase in what are called functional disorders of the nervous system. These are met with in all classes of society and under all conditions, and they come under the observation, not merely of the neurologist and alienist, but almost always in the first instance of the general practitioner.

A recent writer in an American journal has said:—"It is a well-known fact that in no department of medicine is the average medical man so poorly trained as in psychopathology. In medical schools, even to-day, no adequate training is given in psychology. Little wonder is it, therefore, that the treatment of functional diseases is unsatisfactory. But physicians should see the reason, and should to a degree feel that they are not wholly without responsibility that patients of this class fall victims to all kinds of quackeries and cults. If the medical profession knew of a reliable treatment for functional disorders, Eddyism, for instance, could never have flourished as it has. Because of the great increase in this class of diseases and the unsatisfactory condition of psychotherapy, a widespread popular interest has been shown of late in this problem, and cults of various kinds, such as faith-healing, Eddyism, and Dowieism, have made tremendous strides."

The expression "the influence of the mind over the body," while understood perhaps in a general way by those who have given enough thought to the subject, yet involves some very difficult questions of psychology and physiology. We are at once brought face to face with the question, "What is mind?" Here we tread upon ground which has been the site of many a battle between philosophers and psychologists of all ages; yet it is a question which we must answer in some way or another, if we would arrive at a clear understanding of what we mean by "mind over body." The very expression seems to imply that the mind is something distinct from the body; that it is a distinct entity outside the physical structure, which exerts some influence over the brain itself, and through it upon all the organs of the body.

But such a view of mind does not commend itself to many psychologists. We find Professor Bain speaking on this point as follows:—"But of mind

apart from body we have no direct experience, and absolutely no knowledge. . . . In the second place, we have every reason for believing that there is, in company with all our mental processes, an unbroken material succession. From the ingress of a sensation to the outgoing responses in action, the mental succession is not for an instant dis severed from a physical succession. It would be incompatible with everything we know of cerebral action to suppose that the physical chain ends abruptly in a physical void, occupied by an immaterial substance; which immaterial substance, after working alone, imparts its results to the other edge of the physical break, and determines the active response—two shores of the material with an intervening ocean of the immaterial.”

And Dr. McDougall, a well-known physiological psychologist, says:—“This recently acquired knowledge of the nervous system forbids us to accept the view that was for a long time generally entertained—the view that the impulses of the sensory nerves came to an end at some spot or spots in the brain, where they stimulate the soul to produce sensations, while the impulses of the motor nerves are initiated in their central ends in the brain by the soul playing upon them as a musician plays upon the keys of a piano. We are compelled to believe rather that the various processes of the brain are of the type of the reflex processes of the spinal cord, and consist in the transmission of physical impulses through channels of great complexity from the sensory to, or towards, the motor nerves, and to believe that all psychical processes are accompanied by nervous processes of this character. The conclusion that all psychical process or consciousness is accompanied by neural process is now generally accepted, and may be regarded as well founded.”

But there is no necessity to assume, as the only alternative to a purely materialistic conception of mind, the existence of any immaterial break between the terminations of the sensory fibres and the commencement of the motor fibres in the brain. We do know that every sensory impression made upon the brain does not necessarily result in a motor response; that the sensory stimulus may remain, as it were, impressed upon the brain and stored up as a sensory image. We know, however, that action may be the result of what used to be called “unconscious cerebration”; that many acts can be performed without careful deliberation or adjustment or any conscious direction on the part of the individual. We know, too, that these actions, which appear to be of the nature of reflex actions, may be inhibited (in whatever inhibition may consist); that sensory impulses may be prevented from producing corresponding movements. Moreover, when sensory stimuli reach the consciousness, there may be an act of deliberation as to whether or not these sensory stimuli are to be allowed to play any part in the conduct of the individual. In other words, the mental attributes of attention, judgment, and free will, may all come into play. I cannot conceive of such mental processes being merely the “transmission of physical impressions through channels of great complexity from the sensory to, or towards the motor nerves,” without assuming that such transmission is directed or controlled in some mysterious way by the action of some immaterial power, which constitutes the “mind.”

It is true that we have no knowledge of such “mind” apart from the nervous system, or of the existence of “mind” apart from body. But while we admit that all psychical processes are accompanied by neural processes, this only means, after all, that every mental state is manifested by some physical or chemical change in the nervous system. It does not necessarily mean that mental processes and neural processes

are one and the same thing. If we adopt the purely materialistic view, then the expression "the influence of the mind on the body" seems at once meaningless. If all mental processes or mental states are the result of the molecular activity of nerve tissue, then it is clear that the "mind" must be materially influenced by alterations in bodily states; but the mental state which does not exist at all, apart from a neural process, can have no influence in determining physical states of the body.

Leaving aside, however, the theories of the psychologist, I propose to direct your attention to certain facts which, in my opinion, illustrate the interaction of body and mind. I propose to show that not only is the mind or mental state influenced by the bodily state, but, conversely, the mental state does influence the bodily state.

First, as to the influence of the body on the mind, it is scarcely necessary for me to remind you of the fact that the state of mind or consciousness may be very much altered by alterations in the physical condition of the nervous system. When we have a progressive degeneration of nerve cells, nerve fibres and blood vessels, we get a progressive decay in the mental faculties; we see this well exemplified in general paralysis of the insane. Or, again, when as a result of neoplasm, or of cerebral hæmorrhage, or thrombosis, we have localized destruction of certain parts of the brain, we may have disorders of the special senses or of speech or of motion, accompanied by more or less enfeeblement of the mental powers according to the site of the disease and the amount of involvement of the associational areas. We assume that the mental changes depend upon the impaired vitality or destruction of the cortical nerve cells and fibres.

But the nervous system may be intact; there may be no macroscopic lesion present; and yet, as the result of the action of endogenous poisons, such as the toxins of fevers and infective diseases—or as a result of some disturbance in the metabolism of the body, such as we see in myxœdema—or in consequence of the administration of certain drugs, as alcohol, chloroform, ether, opium, and the like—we get a variety of mental states produced, conditions of depression, exaltation, delirium, delusions, and hallucinations. But we are as ignorant of the exact way in which these various physical agents, acting on the nervous system, produce variations in the psychical state as we are of the method of production of nascent thought from brain cells.

We know experimentally that the application of certain poisons to nerve cells and nerve fibres causes definite changes in the power of the conduction of nerve impulses and in their electrical condition, which may be only temporary and recoverable if the toxic agent be soon removed, or, if the action of the latter be more prolonged, may continue and even pass on to actual death of these structures. We must assume that in the body these toxic agents produce the same morbid conditions of nerve cells and fibres, and we attribute the changes in the mental state to the physical effects produced upon the cortical cells and fibres. It is assumed that, since the altered psychical states always ensue when these physical agents operate on the brain tissues, they must be related as cause and effect; and no doubt it is generally true that, when one series of events invariably follows upon another series of events, the two series stand to one another in the relation of cause and effect. But while we admit that all psychical processes are accompanied by neural processes, the nervous system being the medium or instrument for the manifestation of mental states, it is obvious that, if that instrument be impaired, either as the

result of organic degeneration or of temporary poisoning, the manifestation of the mental state must be defective—in other words, we get a distorted view in an imperfect mirror.

But we have also to bear in mind that our conscious mental states are determined by impressions of sensations of various kinds received through the physical agency of the nervous system. We have no knowledge of the things around us save by the activity of our receptive sensory organs. If, then, as a result of temporary or permanent disease, the afferent sensory paths and the efferent motor paths are functionally impaired, we get erroneous impressions from without, and the efferent response is accordingly defective. The delirium, delusions, hallucinations, &c., are really the outward manifestations of a disordered nervous system—the harmony of the whole is lost—the nervous processes are over-active in one part of the brain, defective in another; one part is stimulated, another part is paralyzed or functionless, and so, as a result of an imperfectly balanced nervous system, we get distorted vision and uncontrolled speech.

No better illustration of this fact can be given than that of a man who is suffering from a condition of mental depression, associated with a so-called liver attack. There is no doubt that there is circulating through his blood some toxin or toxins derived from his stomach or intestine. These physical agents circulating through the brain must influence the condition of functional activity and nutrition of the nerve cells and fibres of the cortex. Corresponding to this physical condition the mental state of the patient is one of depression; everything irritates him; he sees trouble on every side; he takes a distorted view of his business; his power of attention is impaired, and his judgments are erroneous. In other words, his mental state is one of perversion, because his impressions from the outer world are received through imperfect and disordered channels; his premisses are false, and he draws wrong conclusions which he is unable to correct; his speech and actions often betray these erroneous judgments. We know how, in such a case, a dose of calomel will make all the difference; in a few hours the poison may be removed from his system, once again his vision is cleared, and his mental state is restored to normal.

But when we come to consider the other side of the question, namely, the influence of the mind over the body, we are met with some difficulties. The materialist who believes all mental processes to be the direct result of molecular changes in the nervous system necessarily cannot admit that mental processes exercise any influence on bodily states. The psychophysical parallelist does not believe at all in the interaction of mind and brain; in his opinion the mental process and the neural process go on side by side, and the one cannot directly influence the other. But a consideration of some facts will, I think, show that, no matter in what particular way the mental state may act upon the bodily state, some such interaction does take place and manifests itself in definite alterations in disease and death.

We have, of course, here, as elsewhere, to be on our guard against the fallacy of the *post hoc propter hoc* argument. I will give you some illustrations. A sudden and severe mental shock may so alter the body metabolism that the patient may develop diabetes. This is illustrated by a case which came under my care some years ago. A boy who was passionately attached to his mother suddenly discovered that she was suffering from cancer. While she was undergoing operative treatment for this, the boy was, unknown to any one, reading up all he could find

about this disease, and came to the conclusion that she would be sure to die before long. This discovery seems to have made a most profound effect upon his mind; from being a bright and strong, healthy youth, he began to lose weight, to become quiet and reserved, and to pass an increasing amount of urine. He began to notice his own symptoms; he read about them, and diagnosed his own case as one of diabetes. He, however, kept this to himself, as he was unwilling that his mother should be further worried. Eventually his symptoms became most pronounced, and in spite of everything that could be done his disease persisted. He watched the downward path of his mother, and followed her to the grave just a fortnight later. From observation and investigation of other cases of this disease I feel convinced that a state of mental strain and tension is one factor in its etiology, but how exactly this result is brought about it is impossible to say.

Take, again, the disease, exophthalmic goitre. This is a disease which we know to ensue not infrequently upon severe emotional shock or excitement. Some writers have drawn attention to the peculiar grouping of symptoms as being a permanent condition of the body simulating that produced by the emotion of fear; the symptoms and appearance of the patient are those presented by a hunted animal. Quite recently I have had under observation a young girl of seventeen years, previously of a nervous disposition, who, as a result of sudden shock, associated with some emotional disturbance, presented the subjective symptoms of this disease—the sense of constriction round the throat, the throbbing in the chest and oppression of breathing, the feeling of tightness about the eyes, &c.—but who did not present any of the objective signs of exophthalmic goitre. We are familiar, too, with the fact that prolonged anxiety and grief appear to determine the development of Bright's disease and of insidious disease of the heart, so that the expression "a broken heart" as a result of grief may be literally true. I think there can be no doubt that these physical conditions may be induced directly not as a result of any physical cause acting on the body, but as a result of the action upon the nervous system of some change in the psychical state which is termed an "emotion." I am well aware of the views of Professor James, and others on the emotions and their physical basis, but it would be out of place here to enter upon a discussion of these views. Personally (and here I am in good company), I cannot help taking what seems to me the more common-sense view, that the psychical state is first in time and the organic visceral sensations are second in time.

But this matter has been, and is still being, worked out in an experimental way by the modern school of experimental psychologists. I would refer you to the pioneer work in this line of Tarchanoff, Sticker, and Sommer, and the more recent and exhaustive work of Peterson and Jung, who, by means of electrical apparatus, have devised a means of showing by fluctuations of a galvanometer needle an indication of the physical changes produced in the body as a result of expectation, imagined or real emotion, or sudden verbal stimuli. These results clearly prove that consequent upon purely psychical stimuli we get actual physical changes in the body, which can be estimated and compared in different individuals.

There is still another aspect of this interaction of mind on body. You are all aware that at any given moment our field of consciousness is more or less limited, in proportion to the degree of attention directed to any particular subject; and, as it were, surrounding that central bright spot of

consciousness we have a halo of sub-consciousness, sensations, and impressions, and at any given time at our will we can bring any part of this sub-conscious area into the active field of consciousness. Normally we ignore many of the nerve impulses and stimuli which are constantly streaming in from the periphery to the central nervous system; we are unconscious of them, yet they are actively at work in effecting muscular co-ordination, in giving information of the state of muscles, bones, joints, skin, &c., which information is being used outside the sphere of our consciousness. For example, as we walk along absorbed in thought or conversing with a friend, these peripheral impressions are actively engaged in enabling us to maintain the erect attitude and to walk steadily; yet we are unconscious of them until some unusual stimulus, in the shape of pain or a tired feeling, directs our attention to them.

But apart from any local peripheral stimulation, if we concentrate our attention fixedly upon any particular part or organ of the body, we become conscious of some sensation in that part. This particular area of the body is brought within the field of consciousness, and we become conscious of the sensory stimuli which are constantly arising from it. If we fix our attention sufficiently long on it, we may experience a degree of pain or discomfort. This is a fact which any one can prove for himself. The exact explanation which I suggest for it may not be acceptable to some, but I do not insist upon the explanation; I only want to use this fact. Now, if any pathological condition be present in any part of the body which causes one to direct his attention to that part, the very concentration of the attention determines an aggravation of the pain or discomfort; and if we are able to divert our attention from the seat of the lesion, then the sensation of pain or discomfort is correspondingly diminished. The concentration of the attention upon any part of the body, especially if the state of mental concentration be accompanied by some emotional distress or fear, or anxiety, may develop a state of limited consciousness which amounts to an obsession. The limitation of the field of consciousness may determine a condition of constant pain or of paralysis, associated with a change in the disposition and character of the patient, so that he becomes absolutely self-centred and selfish.

I will give you some illustrations. A young man of 24 years of age, a clerk by occupation and entirely dependent upon his own exertions for his living, was sent to me by a medical man complaining of "neuritis of the right arm." He had previously been a strong, healthy and athletic man, though of a somewhat nervous disposition, and had never suffered from any illness. Some eighteen months before I saw him he had been exposed to cold, and got a slight amount of muscular rheumatism in the right shoulder. This for a time interfered with his athletic exercises, though it did not prevent him from doing his work. As the pain did not go away all at once he began to think more about it, more especially as he had never had any illness before. The more his attention was directed to his arm, the worse the pain became, and it gradually extended down his arm to the forearm and hand. He then began to fear that this pain meant something serious, that his arm would become paralyzed, and so he would not be able to earn his living. This state of emotional disturbance, combined with the concentration of his attention upon the limb, aggravated his condition; in spite of treatment he got worse, ceased to take any exercise, and finally left off his work, because of the pain in the arm and the feeling of inability to use it. His emotional state had been further aggravated by his being told that he was suffering from "neuritis," and this had

preyed on his mind. A very careful examination of the arm showed that he had no real loss of power at any joint or in any muscle—there was no muscular wasting, and no disturbance of sensation. In other words, there was an entire absence of any symptom of neuritis, and it was obvious that the condition was one of purely functional disturbance due to his mental state. The fact that he was cured in two days proved the accuracy of this diagnosis.

Take another case, slightly different. I was asked to see a woman, 64 years of age, who, I was informed, was suffering from rheumatoid arthritis, and had recently had some fainting attacks. She had always been previously a healthy active woman, living in the country, and always interested in her home and family. Some eighteen months before I saw her she had had a slight attack of rheumatism in one hand, but this had passed off in a few days. Twelve months later she sprained her ankle; this, of course, necessitated her lying up for a time, and interfered with her active life. She then again began to have rheumatic pains in her joints, this time rather more severe, and, as the pains persisted in spite of treatment, she was advised to take a trip to the seaside. While there she consulted a medical man, who told her her only hope of cure was to have high-frequency electricity. Acting on this advice she underwent a course of treatment of this nature for a fortnight, got no better, and returned home in a very depressed state of mind, being convinced, from the failure of the electricity to cure her, that she was incurable, and that her active life must come to an end. She was advised not to use her hands at all, and so went to bed, and apparently grew weaker and more helpless; did nothing for herself, and was waited on most patiently by a devoted but rather unwise nurse, and a family of devoted daughters. She then began to have what were called fainting attacks; but her medical attendant never saw one of the attacks, and, even when he was hastily summoned to see her in one, he always found the heart steady and regular, and the pulse good.

When I saw her with her medical attendant she was lying in bed propped up, with rather an anxious expression. A very careful examination revealed a perfectly normal heart and good arteries, with a pulse of good volume, regular and steady at 76 to 80 per minute. An examination of all her joints showed practically no limitation of movements, and no swelling, and all movements at all joints were possible, though performed with some sluggishness. There were absolutely no signs at that time of any joint disease, but her muscles were flabby and weak. It was said that she could neither stand nor walk nor use her hands. She was inclined to be emotional. An additional factor at work, besides her anxiety about herself, was this: her favorite son was to be married in about a month's time in the neighbouring town, and she was much disappointed because she felt sure she would not be able to attend the wedding.

Here we have the picture of a woman previously of active habits, who, as a result of a sprained ankle, is laid aside, and owing to the disturbance in her body metabolism in consequence of enforced idleness, gets a manifestation of rheumatism in some of her joints. This seems to have depressed her, and I suppose she pictured herself to herself as an old crippled woman, who would henceforth be not fit for much. Then this state of fear and anxiety about her condition was much intensified by the unwise advice given to her, that a certain line of treatment was her only hope; and when this failed to relieve her, as was to be expected, considering the real cause of her suffering, she became further depressed and quite

hopeless about herself. The sympathy of her attached family aggravated her emotional state, and so she gradually passed into the condition I have described. Yet, as I have said, a careful examination failed to reveal any physical condition which would account for her state. The fact was that she had passed from a life of unselfish activity to one of absolute self-centredness and reliance upon others. So far as her physical condition was concerned, there was no reason why she should not be up and about. Being fully persuaded of this in my own mind, I insisted upon her rising from her bed and walking across the room, and sitting up in a chair, assuring her that her fears about herself were groundless.

The subsequent history of the case quite bears out the correctness of this opinion. A competent masseuse nurse was put in charge, and the case fully explained to her. Now this patient will do for the nurse almost anything she is asked; she uses her hands for working or sewing, and feeds herself quite naturally; she walks about, and has had no more fainting attacks. So long as the nurse is there she is able to do these things; but when she is left to her daughters' care she falls back again and complains of her weakness and inability to do things. Her pains are very largely due to her state of mental concentration, as there are no further signs of joint disease. This is a most striking case of the effect of concentration of attention upon a system of the body the seat of slight initial pain, combined with the emotional states of fear, anxiety and disappointment.

The following is another case, in which the site of the morbid sensation was the stomach. A clerk, a married man, aged 34 years, came to me complaining of "indigestion and ulceration of the stomach." He had been suffering on and off for the last six or seven years, and had been under medical treatment practically all the time, but had been getting worse for the last three years. He told me that the pains in the stomach were very severe at times, but were very variable. One fact, however, of importance was that the pain was always relieved by food (a sign, you will say, of duodenal ulcer). This was an invariable rule, the pain was never made worse by food, and he had never vomited. On examination, I found him to be rather anæmic and haggard looking; he had lost weight considerably during the past two years. Physical examination failed to reveal any signs of organic disease anywhere. There was absolutely no tenderness over the stomach or duodenum or liver.

It was quite obvious from his manner, and from the occasional remarks he made, that he was in a very depressed state of mind about himself, and feared his condition was incurable—that he would have to go on suffering, gradually getting worse, till his ailment should finally carry him off. On the day, Tuesday, he visited me, he said the pain was severe; the previous day, Monday, he had not had it, except in the morning; yet he had had the same kind of food as on the day before, *i.e.*, on Sunday. On the Sunday he had had the pain several times during the day. It was clear that there was some condition present which determined the absence of the pain on the Monday. And it was this: on that day a favorite relative had come down from the country, and my patient and his uncle went off to a race meeting, and were much interested in the sport. The patient ate his dinner and experienced no discomfort either before or after it. On returning home in the evening he had a good tea, and spent the evening chatting over old times with his uncle. He had had no pain all day. Next day he had to return to work—the old conditions were resumed, and at once his mind reverted to his condition and his prospects, and his pain returned.

Now, clearly the man's state of mind was this:—"I have an ulcer in my stomach" (his condition had previously been diagnosed as one of ulcer of the stomach); "I am afraid to eat much for fear of making it worse, and for fear it will bring on the pain again. This pain is so severe and constant, and I have been so long under doctors without getting better, that the disease must be incurable. I have a wife and four children; I shall soon be unable to work, and they will be unprovided for." He practically admitted that some reasoning of this kind had been passing through his mind. I pointed out to him that his pain was largely the result of his mental anxiety and the concentration of his mind upon his stomach; that he had no reason to have any such morbid fears as he had expressed, and that, with improved nutrition and a diversion of his mind from his stomach, he would get quite well. He left my room in a very different frame of mind, and returned a few days later to say that he was eating now ordinary food, and had practically lost all his pain. I asked him to come to me again whenever he had any return of his pain; as I have not seen him since, I conclude that he remains well.

Another case was that of a young married woman of 30 years of age, who came to me complaining of rectal pain for the past nine months. She came of a nervous stock. She had had some perineal tear, and slight prolapse of the rectum since the last confinement, some thirteen months previously. When the child was a few weeks old she was subject to prolonged anxiety owing to the child's delicate state, and lost her rest at night. On one occasion she was alone in the house, and fancied that the child was dying. She got then sudden pain in the rectum. Subsequently, an operation was performed for the ruptured perineum and the prolapse, the idea being that the pain was due to this morbid condition. The operation was completely successful from the surgical point of view, but the pain persisted as before. Further anxiety over another of her children tended still further to depress her nervous system. The constancy of the pain, combined with the variability in its exact position, and its burning character, along with an absence of any local condition or disease which might cause it, at once suggested the mental origin, and she was soon cured of her complaint.

Another very striking case was that of a young woman of about 25 years of age, who came to me complaining of some of the subjective symptoms of heart disease. Her history was that she had always been healthy; there was no history of preceding rheumatism, and she had never experienced any symptoms referable to her heart until a few weeks before I saw her. One afternoon she had been playing tennis, a thing not unusual for her to do, but afterwards had had some sensation of pain or discomfort in her chest, which induced her to consult a doctor a day or two later. On examination the doctor told her that she had heart disease, and from that time she began to have symptoms. Now, of course, one might have said that on this occasion she had over-exerted herself, and had produced some temporary dilatation or heart strain. But when I saw her, I found that she had most typical signs of mitral stenosis, with good compensation, and absolutely no sign of irregularity or disordered action. The fact was that her attention had been directed to her heart, and she began to have these subjective symptoms, of which she had previously had no experience, although the character of her heart lesion showed clearly that it must have been there for at least many years. Here, of course, the concentration of the attention, along with the state of anxiety almost always associated with a fear of heart disease, was sufficient to determine her condition.

In all of these cases, you will notice that the seat of the discomfort or pain which determined the condition of mental depression was peripheral. Not infrequently, however, we find that the brain itself is the site of the obsession, and this concentration of the attention upon the nervous system aggravates very materially the condition of mental depression. The following case will illustrate this: A single man of about 38 years of age had been engaged in clerical work for many years. He had always been of a bright disposition, with no excesses of any kind, but had always worked very intently. He had had a fair amount of work, and had had to do two men's work for some time previous to his breakdown. About eighteen months ago, after working late at his office, he returned home about 10 o'clock, and went to bed. At about 2 a.m., he woke with what he describes as a terrible headache, which so alarmed him that he at once got up and went off to see a doctor. In the course of the same day he went to his office, but could do no work, and he has done none since.

When I saw him first, now some months ago, he was looking physically well, and a very careful examination failed to reveal the existence of disease in any organ of his body, while his nervous system to objective examination appeared to be intact. Similar examinations at later periods have always revealed the same condition. But he complained himself of a certain amount of physical weakness, especially on exertion; more especially he complained of a feeling of numbness or of chaos, as he described it, in his brain. The effort of talking for any length of time, or the effort of writing a letter, always ended in a feeling of chaos in his brain, which made him feel that death would be preferable to any further existence in that state. He could not read anything but the lightest literature. His memory was unimpaired, and his mental power was, I believe, also unimpaired; but the attempt to carry on a conversation or to write an important letter produced this feeling in his head, which was so distressing to him that he would do nothing at all which involved any effort of thought. In consequence of his condition he left home, and has no desire to return to his friends, as he prefers a life of quiet and rest away from talk and excitement of any kind. He has been making a slow but a steady improvement.

Now, I consider that this patient is suffering from an obsession of his nervous system, which prevents him from seeing anything else; in other words, he has become absolutely self-centred—the brain being the centre of his field of consciousness. Here we have a man who was previously bright, active and healthy, though always taking a great deal out of himself in his work. As a result of overstrain, he suddenly gets an attack of severe pain in the head, which makes a profound impression on him; he quite thinks that his brain has given way, and that he will never again be fit for his work. These ideas no doubt at first filled his mind, and every subsequent effort to use his brain being followed by the same strain of head sensation, he became quite decided in his own mind that his case was hopeless, and was unable to dissociate his mind from concentration of attention upon his condition. This has served to keep him in the state in which he still remains, though it is not so pronounced. There is no indication of any disturbance of body metabolism, and the case appears to be one of obsession of brain upon the mind.

The following is another case illustrating the same point. A young woman of 20 was brought to me by her mother complaining of queer sensations in her head. She had no headache, but complained of loss of power to take interest in anything. If she read a book she read it

mechanically without being able to grasp its meaning or to remember it. She had learnt to play the piano and to sing, but had quite lost all interest in her music. She did her housework in the same mechanical, uninterested way. If she went to a concert or the theatre she was unable to enter into the amusement, and was glad to be home again. Latterly she had developed a nervous dread of going out by herself, and would not walk up the street or go in the ferry steamer unless her mother was with her. These numb sensations in her head were always present. She had lost in weight slightly, and complained also of attacks of flatulence. She was evidently very nervous about herself, and admitted to me that she was afraid she was going mad. She was constantly thinking about her brain condition.

Her previous health had been good. But at the age of fifteen she began to suffer from attacks of flatulence, which were accompanied by a good deal of flushing of the face; from the description given, I take it that these were really of the nature of a gastric neurosis. Her father had suffered from rheumatic fever, and had died suddenly many years ago. He was evidently what one would call a highly strung excitable man. Her mother is healthy, but a maternal uncle suffers from epilepsy. In April, 1907, she returned in good health from a six months' visit to a neighbouring State, and shortly afterwards became practically engaged to a young man whom she had known from childhood—an actual engagement was not consented to by her mother owing to the youth of her intended; the patient herself, however, quite acquiesced in this. The following winter she came out into society, and went through a fair amount of social excitement. It was soon after the close of the winter season that she began to suffer from the symptoms I have described. She was under treatment by a doctor for ten weeks, and had massage for seven weeks. She had also been taking a large amount of milk and had gained in weight at first, but lost it again, and her weight varied from time to time. She used to weigh herself frequently, and was always worried if she found that she had lost any weight. At the end of this treatment she was practically no better—in fact, in some ways, she was worse, and her symptoms had become more pronounced. Physical examination did not reveal the presence of any organic disease, or any objective evidence of disturbance of her nervous system.

I think the course of events in this case is clear. We have a girl who had always been somewhat delicate, with an unstable nervous inheritance, who became the subject of a gastric neurosis at fifteen years of age, beginning to experience the emotional excitement associated with a love affair; then she has the physical and mental excitement associated with her first season in society, followed by a period of reaction, a state of physical and mental exhaustion. This naturally manifested itself in a loss of energy, accompanied by a feeling of inability to do what she had been accustomed to, and a concomitant state of anxiety from not being able to understand her condition. Then the development of queer sensations in her head, following on the state of brain weariness, induced further morbid fears, and, these fears being unexplained to her, she gradually drifted into a condition of mental depression which was steadily becoming more pronounced. The treatment she had had apparently failed to relieve her, because the cause of her sensations had not been explained to her. In other words, the mental factor in her case had not been seriously considered. In this case we see again the obsession of the brain upon the mind.

In describing these cases I have intentionally avoided discussing the diagnosis, because that is no part of my purpose at present ; my only object is to show that the mental factor took a very large, if not the largest share, in determining the morbid condition.

But I wish now to direct your attention to certain forms of nerve disease which we call "functional," such as hysteria, neurasthenia, psychasthenia. In these morbid states we get a great variety of symptoms, both objective and subjective, and there is no corresponding lesion in the nervous system, central or peripheral. In hysteria we get a syndrome which includes a variety of symptoms and sensations, paralyses, spasms, contractures, anæsthesia, paræsthesia, disturbances of the special senses, &c. The very variety of the symptoms of itself is a conclusive proof that no organic change in the nervous system exists, for none could possibly exist, and life be maintained; and no one nowadays seriously considers that this morbid condition is other than a peculiar alteration of consciousness, which is apparently allied to that artificially induced state of abnormal consciousness called hypnotism.

But with regard to the other diseases referred to, viz., psychasthenia and neurasthenia, the mental factor is not quite so obvious. Nevertheless, it is a most important factor in the etiology of these morbid conditions. To begin with, they are often induced as a result of stress and strain on the nervous system. The business man who has been burning the candle at both ends, finds his nervous energy becoming exhausted. He is no longer able to do his work with his accustomed freedom; everything becomes a burden to him, and he loses interest in things around him. You are all familiar with other types of neurasthenia; the man who is suffering from a chronic flatulent dyspepsia with atony and dilatation of the stomach; the woman who has a floating kidney and knows it, and who suffers from a variety of associated pains and aches and gastric disturbance; and lastly, the sexual neurasthenic who has read all the deceptive advertisements of the quack, and who comes to us in the depths of despair. In all these conditions we have to recognise the existence of a physical state which may be a source of toxins or poisons that disturb the general body metabolism, and so probably poison the central nervous system. In this connexion, we notice, of course, the large amount of phosphates and the presence of indican in the urine in cases of this nature.

But while we recognise the physical factor as a depressant of the nervous system, we must also take note of the patient's morbid mental state. It is important to recognise this fact, because in the treatment of the neurasthenic or psychasthenic, however much we may improve the physical condition of the body and restore the patient to normal physical health, he still remains a neurasthenic to a less or greater degree in proportion as the treatment has been accompanied by suggestion or not. If the mental state has been treated either intentionally by some special method, or incidentally by change of scene or environment, then the mental obsession is diminished, and the patient becomes less self-centred. The mental factor in these cases renders them all the more difficult to treat, and the most difficult of all are the cerebral and the sexual neurasthenics. The functioning of the sexual organs is so closely associated with emotional conditions that their obsession becomes most intense and difficult to overcome. So, too, the dread of insanity in the popular mind renders the obsession of the brain on the mind of the cerebral neurasthenic a most troublesome condition to deal with.

You will recognise, then, that in a very large number of our patients we must take into consideration the mental factor, not only in cases of nervous disease, but in cases of all kinds of visceral disease. There is no doubt that many cases of hysteria and neurasthenia are on the borderland of insanity, and I cannot help thinking that there is a very close relationship psychologically between the obsessions of the neurasthenic and the systematized delusions of the paranoiac and the chronic delusional insane patient. It seems as if the free interchange of ideas, the free play of the mental processes had been interfered with, and the nerve paths for the reception of sensory impressions and the efferent path for the expression of ideas or movements had been short-circuited, so that every sensation received in the brain would pass along the short circuit and become at once an efferent impulse by direct reflex action without the integrating action of the higher centres. As we know that the prognosis is bad in the case of the chronic delusional patient or the paranoiac, if the delusions have become fixed and systematized, so I think that the prognosis for recovery is bad in the hysteric and neurasthenic unless these obsessions are taken early and treated successfully. We are disposed to regard much too lightly the functional nerve pains and symptoms presented by the so-called neurotic patient. We too often overlook the bad inheritance of the patient, the existence of chronic constitutional disease in the parents, such as cancer, tuberculosis, heart disease, &c., and the still more important hereditary influence of insanity, epilepsy, alcoholism, and diabetes—factors of the utmost importance in determining the development in the offspring at puberty or later in life of those functional nerve diseases, which are often more mental than physical. These morbid states need to be treated in the earliest stages with the most scrupulous care and on the most scientific lines, if we would avert the disaster of the patient's becoming hopelessly incurable.

Recognising, then, the importance of the mental factor in various morbid conditions, we must next consider how we may best treat this element.

First of all, we must make a complete examination of the patient and find out, as far as that is possible, the exact condition of every organ of the body. This is necessary in order that we may be able, in a definite and confident way to speak from our own knowledge of the patient's physical condition; and the more careful and complete our examination, the more satisfied will our patients be that we can speak with the fullest confidence on their case. This of itself will tend to inspire a degree of confidence which nothing else can give; it is, moreover, essential not merely as a means of inspiring our patients with confidence in us as their medical advisers, but because it is necessary that we should treat and correct as far as possible every physical defect and functional error. More especially is this necessary in the case of those functional nerve diseases to which I have already referred. To this end I need only mention here the importance of correcting errors of refraction, the correction of nose and ear defects, the securing of oral asepsis, the correction of gastric and intestinal atony by judicious dieting and the use of intestinal antiseptics, &c. It is of course specially necessary for us to be on our guard against mistaking sensations referred to any particular part or organ of the body for symptoms of actual disease in that region.

And here I think it is necessary to utter a word of serious warning against the performance of operations for the supposed cure of functional nervous conditions, more especially on the sexual organs of either man or woman, unless there be some defect or derangement which is obviously

dangerous to life or which must seriously inconvenience the patient. I am confident that many of the cases which are continually in the hands of the gynæcologists for one operation after another are the victims of obsessions of their sexual organs, and the more operations they undergo the worse they become. I hope I shall not be misunderstood. I make no insinuations against the honesty or competency or skill in diagnosis of our gynæcologists, and quite recognise the immense strides which have been made in this department of surgery in the relief of many conditions which in the past must have made life unbearable; but I am quite certain that the performance of such operations as ovariectomy for the supposed cure of hysteria or hystero-epilepsy, or curetting for some forms of functional dysmenorrhœa, &c., are not only not justifiable—again I say, in the absence of recognised organic disease—but positively harmful to patients of the class I have referred to. Moreover it is important to bear in mind that the performance of operations of this nature for serious organic disease is not infrequently the cause of a severe shock to the nervous system, and may be followed by the development of serious functional nervous or mental disease.

In the second place (though not necessarily second in time or importance) we must direct our attention more especially to the mental state, and must consider carefully in each case what it is we have to do. We want to decentre the patient from his own sensations and feelings—in other words, to enlarge his sphere of consciousness and to relegate to the sub-conscious the numerous subjective sensations and feelings of which he complains, and which loom so largely in his imagination. Hence we have to utilize and strengthen his mental faculties. His power of attention, which is so often acutely active in one direction, must be diverted; his judgment must be appealed to, and his false conclusions pointed out to him; his will, which is often dormant, for he is the prey of indecision and morbid doubts and fears, must be aroused; his imagination, which is often lively for all that is unfavorable and of bad omen, must be turned in another direction. In short, all his mental faculties must be investigated, and the line of mental therapeutics must be directed in accordance with the nature of his mental state.

We must remember that the mental state of the patient is often one of increased susceptibility to suggestion; the mere questioning about a symptom will direct the attention to that particular function, and may ultimately determine the development of a symptom which has previously been absent. Then we find further that, owing to the limited field of consciousness and the existence of an obsession, the patient's judgment is erroneous, and he is unable to follow our argument; the condition of brain which I referred to just now as "short-circuiting" prevents his higher centres from diverting his train of thought from the one particular line of transit. Then the moral nature is perverted, and selfish instincts drive a man to consider his own comforts and his relief from suffering as the most important thing in the world, to the neglect of all other duties, domestic and otherwise. He becomes selfish, irritable, and fretful, causing endless misery in his own home, yet perhaps all unconscious that he is himself the main cause of it all.

There are practically three methods of directly treating the mental element in these morbid conditions. I will put out of court at once, as unscientific and unfair to the patient, the method of browbeating and putting off the patient by telling him that there is nothing the matter with him—that he is only imagining his pains and aches. We may rest assured

that these sufferings are as real to him as if he were suffering from organic disease, and are often more severe. It is no pleasure to a patient to be sick and complaining and unable to do his work; we must recognise this fact, and act, if not express, our sympathy with him. Equally wrong is it, of course, to allow such sympathy to exert any influence in magnifying the patient's ailments in his own eyes.

The three methods of treatment are (1) Persuasion, (2) Suggestion, and (3) Hypnotism.

1. *Persuasion*.—By this I mean a careful rehearsal of all the facts in the family history, and the past and present history of the patient which may have any bearing upon the causation of his disease, an endeavour to explain fully and clearly to him the reasons for his condition, and a little exposition of the influence of mental concentration in aggravating his pains and discomfort. This simple method I have found to be extremely useful and helpful to many patients; but of course it has its limitations, and can only be effectively employed in a certain class of case.

In the case of the clerk with the apparent loss of power in the right arm, whose history I related to you a few moments ago, I described to him the progress of events—the gradual evolution, from the emotional condition of fear, of the actual condition of pain and apparent loss of power in the arm. I then pointed out to him that all the signs of organic disease, such as neuritis, were entirely absent, and persuaded him that his fears and anxieties were groundless. He was convinced, and in two days was quite well. Previously he had had massage and other treatment, which not only were of no avail but had rather aggravated his trouble, because they tended to confirm to his mind the idea that he had some definite organic disease.

Then, again, to the young woman who had the dread of going mad because of the queer sensations she experienced in her head, I explained fully the progress of events, their gradual development, and the causes at work. In this case, too, prolonged treatment with massage and overfeeding was of no avail, because of the failure to recognise, or at any rate to treat, the mental factor. The one visit was sufficient to improve her condition, so that she had no occasion to consult me again. I have seen her since, and she is much improved, yet owing to her doubtful heredity I am not sanguine of a complete cure.

The same line of treatment was adopted in the case of the woman suffering from rheumatism. A full explanation of the case to her friends, and a clear and firm statement of her actual condition to herself, was sufficient to effect a considerable amount of improvement; but owing to her age and to the prolonged period of mental concentration and emotional disturbance before I saw her I do not think she will ever be quite well. This procedure takes time; nevertheless, I am quite convinced from my own practical experience that often a great deal more good is thus effected in relieving the state of nervous anxiety and tension than in prescribing an endless amount of medicine or giving exact details as to some particular line of dieting. The treatment by persuasion appeals to the patient's reason and his competency to form a judgment upon the facts placed before him, and may need to be repeated at frequent intervals.

2. *Suggestion*.—By the method of suggestion we appeal to another side, as it were, of the mental faculty. We try to arouse the patient's power of anticipation; we endeavour to secure the patient's confidence in our full understanding of his condition, and in our knowledge of the natural evolution of disease; and we suggest to him that the performance of certain

actions, the adoption of some special line of treatment—whether it be massage, electricity, various forms of baths or a sea voyage—will inevitably result in his recovery. This method, again, is most useful in certain cases, yet has its limitations. It is the method of the arrogant and ignorant quack, who, by his strong personality and emphatic and repeated assertions of his power to cure the disease from which the patient is suffering, exerts a profound influence over the suggestibility of the nervous sufferer, and thus is often able to effect a cure—or, at any rate, materially to improve the condition of the patient whose sufferings are largely the result of auto-suggestion and mental obsession, and who failed to get relief from the qualified practitioner because the latter has not recognised or does not know in what way effectively to deal with the patient's mental attitude.

Unfortunately, this method of treatment is often known to patients as that adopted by the quack; and, when the medical man attempts to use it by giving a placebo and suggesting to the patient that the medicine will be sure to cure him, he earns for himself the epithet of "quack." But this is no reason why we should not utilize the process to the fullest extent. As a matter of fact I suppose every practitioner does so more or less by encouraging patients, who are suffering from various forms of disease, in the hope of ultimate recovery. In special cases, however, the method requires to be used with caution, lest by suggestion the patient's attention become more fixed upon his special symptoms. Nevertheless the method can be availed of in treating many conditions. The sufferer from constipation, if he can use our suggestion that by going to stool at a certain hour every morning he will secure an evacuation, may be cured of his trouble; the same result may ensure to the sufferer from insomnia, on the suggestion that if he retire to bed at a certain hour he will be sure to sleep, and the dyspeptic who protests that he cannot digest milk in any shape or form can be made to digest it on the assurance that milk is the most easily digested food, and that if he cannot take milk no other food will agree with him.

This method can also be made use of in the treatment of alcoholism and drug habits, the suggestion being made that, after a certain line of treatment has been adopted, the taking again of alcohol or other drug will cause him to vomit, and being intensified by the simultaneous administration of apomorphia. This principle is also at the root of some methods of treatment of hysterical attacks of various kinds. The mental impression or obsession of the hysteric that he is anæsthetic or paralyzed or cannot speak is corrected by the auto-suggestion created by the use of a powerful faradic brush, combined with the suggestion that after the application of the electricity the paralysis or anæsthesia or aphonia will be cured. This element of suggestion is also a most necessary adjunct to massage and electricity in the treatment of cases of neurasthenia and psychasthenia; and herein lies the necessity that a successful masseur or masseuse should be a strong personality with every confidence in his or her own power of doing the work well, who can daily during the treatment also use suggestion. Apart from this, massage and electricity alone may effect some improvement in the nutrition, aid digestion and favour sleep, but will be insufficient to effect a cure.

3. *Hypnotism*.—This is, of course, a more profound degree of suggestion; but we are entirely ignorant of what hypnotism actually is or what happens when the patient is hypnotized. We know, of course, that the mental condition of the hypnotized person is one of eminent impressionability by suggestion. The condition is clearly one of automatism; the patient for the time being is like an animal whose cerebral hemispheres

have been removed and which can perform any movement when forced to do so, but is incapable of initiating any voluntary movement. Impressions made upon the mind of the patient in this state are retained in the state of waking consciousness. By this method no doubt many wonderful cures have been effected, and it is said to be of distinct benefit in the treatment of the drink and drug habits.

But while I am prepared to admit the value of the method in some cases, I do not accept without the proverbial grain of salt the records of some of the writers on this subject. I feel sure that if these reports were correct, and the results obtained were permanent, we should certainly be fast hastening on to a golden age when sickness and suffering would be things of the past. The fact that this mysterious power which can be exerted by one individual over another has been known for many years, has been utilized in various ways, and yet has never been accepted by the general body of the profession as a safe and certain method of treatment, proves that, rightly or wrongly, the general feeling of the profession is against the use of a method which appears to be fraught with such immense possibilities for evil as well as for good. Personally, in spite of all that can be said of the harmlessness of the procedure, I regard it as a method to be used only in exceptional circumstances, and in the most guarded way, feeling convinced that we can secure as good results, though perhaps not so quickly or so dramatically, by the act of waking suggestion or persuasion.

The discussion on hypnotism at the annual meeting of the British Medical Association at Exeter in 1907 entirely bears me out in the views I have expressed on this subject. I think that the record of the very large number and varied character of the cases reported to have been cured by hypnotism by the opener of the discussion on that occasion is of as much value as the record of the cases of cancer cured by Chian turpentine by the Birmingham surgeon of the last century; and the confession of one of the speakers, who twenty years ago was an enthusiast in hypnotism, that his ardour had been considerably cooled by his later experience of frequent failures, is certainly worthy of note. Still I am sure that none of us will cavil at the use of hypnotic suggestion if by its means the slave of alcoholism or of drug habits can be freed from his chains and restored to normal mental control.

In conclusion, then, I would again urge the necessity of remembering that the human being whom we have to treat is different from the lower animals in possessing mind as distinct from instinct; that in every case of every disease we have to study carefully the influence of the mental factor in determining the character or the persistence of certain phases of the disease, or of certain symptoms and sensations; and that, unless we do take note of this factor and do treat it effectively we shall often fail to cure our patients, and shall find them figuring in the records of the cures of the faith-healer, the Christian scientist, and the quack. If these remarks lead to a more careful consideration of this question by every practitioner in every department of medicine, my object will have been attained, and the goal of our treatment, *mens sana in corpore sano*, will be nearer accomplishment.

SYPHILITIC NEPHRITIS.

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Very little attention has until recently been paid to syphilis as a possible factor in the causation of kidney trouble, except in the case of amyloid disease, and many text-books of medicine quite ignore it as a cause. Still, there is a gradually increasing amount of evidence as to there being such a condition as syphilitic nephritis, and Osler gives the proportion of syphilitics who develop nephritis as 2.8 per cent. I propose to read notes of three cases which I consider as fairly coming under this category, and to make a few comments on them.

Towards the latter end of 1889, A.B., a medical man, was infected on the finger with a hard chancre. The condition was not recognised until secondary symptoms, rash, adenopathy, and tonsillar ulceration had developed. Then treatment with the pill of hydrarg. c., creta gr. i., and pulv. ipecac. co. gr. ii., was at once adopted. All symptoms soon yielded, and did not recur.

Several months later, the patient, being regularly under mercurial treatment, dropsy of the legs was noticed. On examination, the urine was found to be loaded with albumen, the specific gravity and the quantity were not altered. A few hyaline casts were found. The patient was always able to keep on with his work. The condition of albuminuria with dropsy persisted for over two years—gradually disappearing towards the end of that time. Although the urine has been frequently examined during the last fifteen years, no albumen has ever been found.

Interesting features in the case are—

1. That the patient has a small scotoma in the left eye, the result of an old hæmorrhage in the retina;
2. That, although he is an abstemious man, and there is no family history of gout, he had, soon after the disappearance of nephritic symptoms, a mild attack of that disease; and
3. That now, at the age of 43, his systolic arterial tension is nearly 150 m.m. of mercury.

This case seems to me to conform in many respects to the description given by Professor Rose Bradford of syphilitic nephritis (B.M.J., 30th March, 1907). This authority has drawn attention to the fact that it is not uncommon for nephritis to occur during the secondary stage of syphilis; it is often overlooked or accidentally discovered, and, if found, it is apt to be attributed to cold, and not to the infection. It is just as likely, however, for nephritis to be a sequence of syphilis as of scarlet fever, typhoid, or measles.

Bradford states that the characteristics of syphilitic nephritis are as follows:—There is intense albuminuria, the urine frequently becoming solid on boiling, and yet the patient may not present very marked signs of illness. Dropsy may occur, and, if so, the disease is more severe in its course. The albumen persists for a long time—six or eight months, or longer—but may completely disappear, and the patient regain his usual health.

A distinction must be made between syphilitic nephritis and mercurial albuminuria (V. Lecorché). In the latter, there is a renal irritation, causing albuminuria, which comes on when mercury in any form is administered, and disappears when the drug is discontinued. Nephritis is often present

when no mercury has been administered, and sometimes, but not always, tends to disappear when the specific remedy is given. It is probably of the type of parenchymatous or diffuse tubal nephritis, and is comparable with the nephritis seen in the course of chronic pulmonary tuberculosis, which Professor Bradford regards as being directly caused by the tubercle bacillus.

That the condition in the case recorded above was actually a nephritic one is made probable by the presence of the eye change; the fact of the development of a gouty attack seems also to indicate the probability of some kidney insufficiency, and the increased arterial tension points to some possible renal change.

A long time may elapse between the syphilitic infection and the first sign of kidney trouble. Thus, Dickenson (*Pathological Society's Transactions*, Volume 30), records the case of a man who contracted syphilis in 1861; in 1874, he had persistent nodes, and soon afterwards became œdematous, his urine being loaded with albumen. In June, 1879, his urine was normal.

In March, 1896, C.D.—a patient of mine—contracted a gonorrhœal attack, which was accompanied by soft sores and epididymitis. Seven weeks later he developed a hard sore at the site of the soft sores, which had then healed. Rash and adenopathy followed, with pains in his bones. He was put on mercurial treatment, but gave it up at once on becoming slightly salivated.

Another attack of gonorrhœa followed in August, and this gave rise to a fresh attack of epididymitis, and left a chronic gleet.

In November he was found to have facial œdema with dyspnœa, the urine was of specific gravity, 1022, loaded with albumen, containing also some granular casts and a few leucocytes.

The dropsy and albuminuria persisted for months, and at first were accompanied by much diarrhœa. The abdomen and pleuræ were repeatedly tapped. Mercury in any form, even as inunction, at once caused severe diarrhœa. After tapping, the liver was noticed to be enlarged, but the spleen was not palpable.

In April of 1897, the patient was given tinct. ferri perchlor. and pot. cit., with some improvement, and eventually, in August, profuse diuresis, up to 174 oz. daily, occurred, and the dropsy quite disappeared. Improvement gradually took place, and the albumen, which had originally been as much as 1.2 per cent., diminished to .2 per cent.

In January, 1898, the patient was attacked by severe diarrhœa. He was ordered a bismuth mixture, and, if the diarrhœa did not cease, a suppository of morphia, gr. $\frac{1}{3}$, was to be used. Although the diarrhœa stopped soon after the administration of the medicine, three of the suppositories were used—suppression of urine followed with coma, and the patient succumbed.

Here the condition suggests an amyloid rather than an inflammatory condition. The time that elapsed between the onset of the syphilis and that of the renal symptoms was short, but the patient had also a suppurative urethritis, and this would possibly contribute to the establishment of speedy amyloid degeneration.

Dickenson has recorded a case in which lardaceous changes were recognisable, *post-mortem*, three weeks after the beginning of suppuration. There was in this second case of mine polyuria, the urine being clear, pale, and usually of low specific gravity, and there was hepatic enlargement; but there were some granular casts, and the progress of the disease and

the tendency toward improvement after many months are characteristic rather of an inflammatory condition. Possibly, as frequently happens, amyloid disease and tubal nephritis were concurrent.

In November, 1893, a young man of 23 was admitted into the Alfred Hospital with anasarca. Four months previously, he had noticed a venereal sore, which was followed by glandular non-suppurating enlargement in the groin and some alopecia. A month before admission, he had had shivering attacks with pain across the loins, and had noticed that his urine was scanty and dark in colour. Then swelling of feet and face came on, and gradually extended. The urine was of specific gravity, 1010, and contained abundant albumen and some hyaline casts. He was put on a mixture of citrate of potash, gr. xx. and infusion of jaborandi $\bar{5}$ i. Two days afterwards, he passed 161 ozs. of urine and 175, 180, and 149 ozs. on the following days. The quantity then gradually diminished to 60 and 40 ozs. On 23rd November the urine was non-albuminous, and the anasarca had disappeared. He was discharged three weeks later, apparently perfectly well.

During his stay in the hospital, there was slight macular rash on the abdomen and some complaint of sore throat, but no distinct syphilitic ulceration, so that there was no very definite evidence of the presence of syphilis, but, here again, the character of the symptoms and their rapid and complete disappearance, together with the history before admission, point to the possibility of the disease being specific. It may, of course, be regarded as a nephritis coming on during the early stage of syphilis and quickly recovering.

A condition that has given rise to much discussion and difference of opinion of late years, is that known as the contracted white kidney, in which we have anæmia, with high pulse tension, marked changes in the optic disc (neuritis and hæmorrhages), and very little dropsy, the condition coming on insidiously in young adults with no previous history of acute nephritis. This has been supposed to be sometimes due to congenital syphilis. Nettleship (*Lancet*, 1st October, 1904), in writing of chronic interstitial nephritis in the young, gives an analysis of 80 cases of this disease; he states that hereditary syphilis was a factor in six of these, and was highly probable in several others.

I have seen several of these cases, and in almost all there was no evidence otherwise of congenital syphilis in the patient, and none in other members of the family. One case has, however, come under my notice in which the brothers and sisters of the patient, both older and younger than her, are quite free from any disease; the patient herself has, in addition to chronic nephritis, a depressed bridge of the nose (non-traumatic), and recurrent non-traumatic synovitis of the knee, which has yielded readily to the administration of iodides. The possibility of syphilitic disease cannot be denied in this case.

The part played by syphilis in the production of the contracted kidney has been supposed by many to be very small. Thus, in Fagge's *Medicine* we read that "There is no belief, as there is no evidence that syphilis is a factor in causing cirrhosis of the kidney." More recently, however, some importance has been ascribed to syphilis in this connexion. Thus, Stengel (*Pathology*, page 669) says—"Syphilis of long duration may lead to amyloid degeneration of the kidneys or to chronic interstitial nephritis. Thickening of the blood vessels is conspicuous. In the latter case, contractions of fibrous tissue leading to marked lobulation are rather characteristic. Butler, again, in *Diagnostics of Internal Medicine*, says

that chronic interstitial ne-phritis may result from arterio-sclerosis, gout, rheumatism, or syphilis. Recognising, as we all do, the close connexion that exists between arterio-sclerosis and chronic Bright's disease, and also the importance of syphilis as a factor in giving rise to arterial degeneration, it seems likely that syphilis may play a not unimportant part in the production of chronic kidney lesions. If syphilis can produce the lesions of a parenchymatous nephritis, it is not at all unlikely that more chronic changes may with much probability be ascribed to it.

The treatment of the condition is that of the syphilis according to the stage reached. Mercury is said to cause disappearance of the nephritic symptoms in the secondary stage, while iodides are of great value in those conditions occurring in the tertiary period, whether amyloid or otherwise.

A word of caution is necessary as to the use of iodides where there is a marked condition of kidney insufficiency. Symptoms of severe iodism are apt to take place, even from small doses, and I believe I have seen two patients succumb from this cause.

THE PROPHYLAXIS OF SYPHILIS.

W. J. A. MOSS, M.B., MELB.

Syphilis, in its varied forms, I felt sure would be dealt with by the distinguished members of our profession from the different States of our Commonwealth and from the Dominion of New Zealand. There was one aspect of this great subject that I thought ought to be discussed, and that is the prophylaxis of syphilis. The prevention of any disease is really of more importance than the treatment of that disease, and it concerns every individual in this community, as well as the very existence and future of the State, of which the individual is but a part. It is one involving great difficulties and complications, which have hitherto baffled the efforts of medical sociologists of all time to combat.

E. L. Keyes, of New York, in his recent work on syphilis, in his opening chapter, says—"Some fifteen years ago there appeared, for the first time in America, a book on syphilis, the last chapter of which bore the title of 'Syphilis in relation to Public Health.' Fifteen years ago the science of preventive medicine was in its infancy, and venereal sociology did not exist. In the intervening years, evidence of the appalling prevalence of syphilis has been accumulating with ever increasing force; and, if evidence of its preventability is lacking, this is due to the prudishness of our morality, the inherent sluggishness of public opinion, and in no small degree to the unwillingness of medical men to take their part in the crusade. The question, therefore, of the prevalence and the preventability of syphilis, which formed an almost surreptitious last chapter a decade and a half ago, now very properly usurps the initial position."

Prince A. Morrow, in his work on "Social Disease and Marriage," writes as follows:—"From the study of the relation of venereal diseases with marriage, it will be seen that these diseases, from their nature and mode of propagation, are intimately mingled with the sources of life. They affect the dearest interests and most sacred relations of human society, and their prophylaxis is of the highest social importance. The vitality

of the race, the health of the family, and its productive energy are involved in the conservation of the integrity of functions which these diseases damage or destroy."

While it is the husband and father who conveys the infection to his family, and who is the responsible cause of the wreckage of the lives of his wife and children, yet all these innocent infections can be traced back to their original source in that irregular sexual commerce known as prostitution. It is evident, then, that the prophylaxis of venereal diseases in marriage cannot be considered apart and distinct from the prophylaxis of venereal disease in general. The peril to the family is but part of that vast venereal peril which so seriously menaces the public health, and which, with alcohol and tuberculosis, constitute the three great modern plagues that afflict humanity.

The prophylaxis of venereal diseases and the prevention of prostitution are indissolubly linked. We cannot dissociate the effect from the cause. In the existing economic and moral conditions of society, it is a necessary evil, not in the sense of being indispensable, but inevitable, and the only relief lies in the correction of the conditions of which it is the outgrowth.

The system of regimentation, which is employed in France and many other countries in Europe, has for its object the hygienizing of prostitution by eliminating all sources of disease in the women who are engaged in it. This was attempted by the official registration of all public women, and their examination at stated intervals by physicians appointed for that purpose.

Commence states that venereal diseases are always far more numerous in countries where there is free-trade in prostitution than in those where regulations are in force.

Statistics would seem to prove that when the compulsory examinations were abolished in the United Kingdom and India, that admissions for venereal diseases were lessened in the United Kingdom—a drop from 263 to 173—and increased in India from 235 to 536. In France, Germany, and Austria, where regulations are in force, on the other hand, admissions are numerically very much smaller than the United Kingdom or India, bearing out Commence's statement already quoted. The fall in numbers in the United Kingdom has been used by some critics as an argument that the abolition of the compulsory examinations has been the actual cause of the diminution, whereas it can be clearly put down to other causes, as the passing of the Education Acts, compulsory school attendances, gradually increasing improvement in the moral character of the whole population.

The Contagious Acts themselves inculcated cleanly habits and the necessity for medical treatment. In India, on the other hand, the same rapid advance in sanitary progress has not been possible, and admissions for venereal diseases have increased to an alarming extent.

Quain says—"The prevention of syphilis and gonorrhœa by periodical inspection of prostitutes and removal of them to Lock hospitals, when diseased, is only carried out in this country in certain military and naval stations, when the effect has been to lessen primary syphilis by nearly one-half, and to abate its virulence. It is most urgently to be hoped the Legislature may before long deal thoroughly with this matter, and attempt to lessen syphilis, not merely in the army and navy, but among the population at large."

Osler, in discussing the subject of the "Prophylaxis of Syphilis," writes—"The second measure is a rigid and systematic regulation of

prostitution. The State accepts the responsibility of guarding citizens against small-pox or cholera, but in dealing with syphilis, the problem has been too complex, and has hitherto baffled solution. On the one hand, inspection, segregation, and regulation are difficult, if not impossible, to carry out; on the other hand, public sentiment, in Anglo-Saxon communities at least, is as yet bitterly opposed to this plan. While this feeling, though unreasonable, as I think, is entitled to consideration, the choice lies between two evils—licensing, even imperfectly carried out, or widespread disease and misery.”

In India, the Lock-hospital system was in force from 1865 to 1884; it was partially suspended in 1884, and was abolished in 1888. From 1888 to 1898, there was consequently a marked increase on venereal prevalence. The Army Sanitary Committee, in March, 1897, acknowledges the effect of the former Acts in India in checking, not only the increase of syphilis, but also the severity of the disease itself.” The Home Government, however, was influenced to abolish the necessary precautions taken for the well-being of the British soldier in India by the sensational reports of certain American ladies visiting India. The result was that so great an amount of preventable disease broke out, that after a few years the new order from Home resulted in a daily amount of venereal disease in hospital, equivalent to the strength of three British regiments. The order had to be modified. Major French* advocates rewards to men showing a clean medical history-sheet over a fixed period as regards these affections.

French says that if we recognise the cardinal fact that prostitution has always existed, and, unfortunately, must continue to do so for all time, since it is primarily dependent on poverty, it is then self-evident that to control disease which is the direct outcome of prostitution it becomes necessary to more effectually control all irreclaimable persons, and to place restraint on the secret spread of disease by women who are known as clandestines. Expense, however, is the chief barrier to more effectual control.

Again, French says—“The localization of prostitution, therefore, is a most valuable factor in the prevention of disease in any place, and control, to be of any real value, absolutely hinges on it.”

In *The Prevention of Disease*, translated from the German, with an introduction by H. T. Bulstrode, we have it laid down that—“The prophylaxis of venereal disease must comprise the prevention of syphilis, of soft chancre, and of gonorrhœa.”

Doubtless all these affections stand in close relation with prostitution. The supervision of this is the only prophylaxis which the State can offer to the individual. The unanimous opinion of all specialists—and my own experience coincides—is that the unregistered prostitutes are responsible for most of the spread of syphilitic and gonorrhœal infection, and yet they have never hitherto had an opportunity of getting medical aid conveniently and without charge.

When on a visit to Japan a few years ago, I was struck with the absence of solicitation in the streets of any of the cities. This was explained by the fact that, both at Yokohama and Tokio (and other cities) there is a portion of the city set apart for prostitutes. From Murray's *Handbook for Japan*, I take the following:—“About 1 mile to the north of Asakusa Park, in Tokio, lies the world-famed Yoshiwara, the principal

* *Syphilis in the Army.*

quarter inhabited by the licensed betairæ of the metropolis. Many of the houses within this district are almost palatial in appearance, and in the evening present a spectacle probably unparalleled in any other country, but reproduced on a smaller scale in the provincial Japanese cities. The unfortunate inmates, decked out in gorgeous raiment, sit in rows, with gold screens behind, and protected from the outside by iron bars. As the whole quarter is under municipal surveillance, perfect order prevails, enabling the stranger to study, while walking along the streets, the manner in which the Japanese have solved one of the vexed questions of all ages. Their method, though running counter to Anglo-Saxon ideas, preserves Tokio from the disorderly scenes that obtrude themselves on the passer-by in our Western cities. The great objection to the segregation and regulation of prostitutes in British communities is the fact that such regulation and licensing by the Government or municipality would encourage prostitution, and not diminish it, as the popular idea would be that little or no risk of contracting syphilis or gonorrhœa need be feared, whilst the prostitutes were under medical and Government control.

On the other hand, temptations to err by means of solicitation in the streets of our cities would be done away with. Prostitution would not obtrude itself in the barefaced way it does at present, and there is no shadow of doubt, in the medical mind at any rate, that syphilis and gonorrhœa would be considerably reduced. If all female prostitutes were done away with to-morrow, as the abolitionists would desire, it would not affect the spread of syphilis, for the male carrier of infection would be left, and other females would soon take the place of those who were debarred from their calling, and so prostitution would go on, and syphilis be spread broadcast as before.

It is only, then, by devising some scheme for the removal of prostitutes to one quarter of a city, or to a place outside a city, constant medical inspection, bi-weekly or tri-weekly, the removal of those who have contracted syphilis or gonorrhœa to a hospital set apart for that purpose alone, that we can hope to prevent the spread of such dire diseases, with all their terrible complication and sequelæ. And not until our legislators and the community are aroused to the need of such legislation as will accomplish this method of dealing with the great social evil, not until then will we see any diminution in their spread and the ravages they entail.

In bringing this paper to a close, I think the opinions of the writers I have quoted, and the statistics they have collected, must drive us to one conclusion, and that is that to effectually deal with the prophylaxis of syphilis is to deal with prostitution, and that this can only be accomplished by systematic regulation and inspection in the manner I have indicated.

MEDICAL SECTION.—TUESDAY MORNING.

DISCUSSION ON SYPHILIS.

DR. JAMIESON opened the discussion with the remark that discussion is difficult, because there must be a mass of contradictions, as we have no statistical basis for statements regarding the prevalence of syphilis, and can only rely on our own experience. In his own experience, based on mortality returns for Melbourne, the rarest thing is to meet a death due to syphilis. Physicians seem to be less impressed than surgeons and pathologists. The pathologist tells us he "sees" and "knows," and we

must take it or leave it. To settle the question, we will have to wait for some serum-diagnosis method with which to test large bodies of the population, and then we shall get a statistical basis. It may be said that round one or two points is found the battleground. In diseases of the nervous system and of the vascular system, take diseases of the cord. We have here an admirable instance of the value of the combined clinical and statistical method, *e.g.*, in locomotor ataxy it may be taken for granted that syphilis is in some way an antecedent of locomotor ataxy. I would say to the pathologist—"Could you have made out that locomotor ataxy has a syphilitic basis?" I would tell the pathologist that if the physician and the statistician had not come in, we would probably never have learned the pathology of locomotor ataxy as being syphilitic. We are not prepared then to let the pathologist take high ground all through. Then take diseases of the vascular system. I have heard it said, "If you get a placenta whose arteries are thickened, that is syphilis." When a man arrives at a generalization as broad as that, he is getting on to a logical inclined plane, and he readily goes on to a further generalisation—that wherever you meet thickened arteries, it is syphilis—that is the danger. We do not want any broad assertions, and must, as far as possible, avoid being misled in our generalisations.

DR. CROWTHER (Tasmania) said: I hope the result of the discussion will be to confirm the importance of sending the history of the case along with the specimen when submitting it to the pathologist for an opinion. I have seen, this morning, a placenta which I was told was syphilitic, but could get no history of the case whatever, and nobody knew anything about it.

DR. MILLS (Sydney) said: In my experience in hospital practice the number of syphilitic cases is not very large, and I cannot believe that the disease is so tremendously rife, even in hospital practice, and much less so in private practice.

DR. STAWELL (Melbourne) said: (1) That to obtain a well-balanced opinion as to the prevalence of syphilis was one of the most important points in the work of the present Congress. (2) That, chiefly under the influence of the teaching of the professor of Pathology, most clinicians in Melbourne had been led to believe that syphilis was overwhelmingly prevalent as a cause of serious disease; within recent years, however, as the result of nerve and clinical experience, combined with further observations on the pathological appearances found in the autopsies at the Children's Hospital, he had modified his views on the subject, and felt in accord with the opinions as expressed by Dr. Jamieson. (3) No one would deny the prevalence and seriousness of syphilis; the question to be determined was the degree of that prevalence; and one of the first points to be decided was whether it was reasonable to accept from the pathologist the statement that certain changes about the walls of arteries, both large and small, were not only characteristic of syphilis, but were absolutely specific. He believed that such a view was not reasonable, he could not say it was not correct, although he could say that during a recent visit to England he found that the younger pathologists from Germany in England and Scotland did not accept even an obliterating endarteritis as absolutely pathognomonic of syphilis, any more than the older pathologists accepted Heubner's views on the same subject; it had been specially pointed out that the same structural changes were to be found, for instance, in vessels round old tubercular lesions as were present in syphilitic endarteritis. (4) He had seen recently a clinical case which exemplified the points: A young delicately-nurtured girl, aged eighteen,

consulted a surgeon about a lump which had formed rather rapidly on the supra-sternal notch; the surgeon suspected sarcoma, and excised a small piece of the lump, sent it to one of the leading pathologists in Melbourne for examination, who reported that the growth was a syphilitic granuloma; the girl was, without question, put on to anti-syphilitic treatment, and got rapidly worse. On his (Dr. Stawell's) return from England, the girl, who had been previously a patient of his for suspected tubercular peritonitis, presented herself for examination; she had a cough and expectoration, and the sputum was found to contain numerous tubercle bacilli, and the apex of right lung was found affected. In this case, the family and personal history gave no sort of suspicion of syphilis, and the whole clinical appearance of the case and its course was one of tubercular adenitis and pulmonary tuberculosis, without any sort of suspicion of syphilis, and yet, on the finding of certain appearances in small vessels, an absolute diagnosis was made. It had to be recognised that here there was no accidental mistake, but a grave error, founded on what seemed to him to be an untrue generalization. (5) The same class of error was probably to be met with in the work of some oculists, who made the broad generalisation that practically all cases of a certain type of choroiditis were syphilitic; several such cases he had seen, where the possibility of inherited syphilis could be reasonably excluded, and where the possibility of genitally acquired syphilis was absolutely, and with apparent truth, denied. It seemed to him that it would have to be recognised that other toxins besides the syphilitic virus produced degenerative changes in vessels exceedingly characteristic of syphilis, and until we knew more it was unwise to be dogmatic. (6) In pædiatric clinical work he had found himself compelled to withdraw from generalizations made in hospital out-patient practice as to the "specific" significance of certain clinical signs; the subchronic enlargement of liver and spleen were, for instance, not always due to congenital syphilis.

THE PRESIDENT said: On summarising the discussion that (1) it was very clear that in the Medical Sections of the Congress considerable doubt was felt concerning the extreme degree in which syphilis was held to prevail by other observers; he personally agreed with previous speakers concerning this point; (2) he was of opinion that the vascular lesions of syphilis could not all be held to be absolutely specific, that their significance must in many instances be deduced from all the facts of the case, and not decided upon the histological appearance alone. (3) His own practice would show that syphilis might almost be ranked as an incurable disease; he was constantly being impressed with the virulence and persistence of the syphilitic poison.

ANÆMIA IN ANKYLOSTOMIASIS AND MALARIA.

R. A. O'BRIEN, M.B., B.S.

The consideration of anæmia arising from malaria or ankylostomiasis is of great practical importance to the practitioner in Northern Australia, and of somewhat less interest to his southern *confrère*, who sees only occasional cases from infected regions. Malaria, almost entirely benign

in type, is found only in the far northern parts of the Continent, but ankylostomiasis occurs sporadically as far south as the northern border of New South Wales. I do not know of any cases arising further south than this.

TYPE OF ANÆMIA IN ANKYLOSTOMIASIS.

Volume and Contents.—The most striking change from the physiologist's point of view is the great increase in the total blood volume. Haldane and Baycott, working with cases of only moderate severity, found an increase of blood volume to 194 per cent., *i.e.*, there were 8.8 c.c. of blood per 100 gm. body weight, the normal being 4.5 c.c.; the increase occurs almost solely in the fluid portion. The amount of hæmoglobin in unit volume, *i.e.*, the percentage by the Haldane-Gower's instrument, is much reduced, but on account of the great increase in total blood volume it is found that the total amount of Hb. in the body is but slightly below normal, the slight deficiency being probably due to direct hæmorrhage from the lesions made by the worms in the bowel wall. In one case investigated the total blood volume was 197 per cent. of the normal, the Hb. percentage by the Haldane-Gower's instrument was 45, but the total Hb. in the body was 89 per cent. of the normal; similarly, the erythrocytes, though reduced to 3,376 millions per unit volume, *i.e.*, per c.c., were actually increased in *total* number to 140 per cent. of the normal.

Oxygen Capacity.—The total oxygen capacity (estimated by Lorrain-Smith's CO. saturation method) is but slightly below normal in severe ankylostoma anæmia, being .74 to .80 c.c. per 100 gm. body weight, instead of the normal .83.

The corpuscles being much increased in total number and the total Hb. slightly diminished, it follows that the colour index falls considerably, so that we have very clearly defined differences from such types of anæmia as—

- (1) Pernicious—in which the colour index is always high and generally above unity, and in which the oxygen capacity is practically never above .5 per c.c. per 100 grms.
- (2) Anæmia from severe hæmorrhage—where the oxygen capacity is always much below the normal .83, often being as low as .39 or .4.
- (3) Chlorotic type—which is not so easily distinguished by any clinically-applicable measures considered thus far, having a much-increased blood volume, a slight decrease in total Hb., and an oxygen capacity of .8. It, however, fortunately never presents the eosinophilic increase constant in ankylostomiasis.

This great increase in volume suggests that the worm secretes a specific poison and injects it into the host, and one wonders for what purpose it has developed this power. Possibly, like the leech's poison, to decrease coagulability and make the blood easier to suck! I cannot find any observation on the coagulation time in severe ankylostoma anæmia, and, unfortunately, I made no observations when I had the opportunity. It would be interesting to know if the bodies of the worms contain any principle that in any way alters the blood picture.

Cytological Examination.—The erythrocytes per c.c. are decreased and the colour-index is low; the leucocytes per c.c., other than the eosinophiles, are increased slightly, but the essential and constant feature is the marked

eosinophilia. On more careful examination of the film, poikilocytes, micro-, and macrocytes, a few normoblasts and, rarely, a megaloblast may be seen in severe cases. Boycott and Haldane, working with cases of moderate anæmia, stated that—"Taking a somewhat elastic view of what constitutes pernicious anæmia, it cannot be said that it is other than an extreme rarity in ankylostomiasis." However, they themselves found several cases with a colour-index above unity, and saw, in a very severe case, poikilocytes, microcytes, and megalocytes, with normoblasts and occasional megaloblasts, and with an eosinophile count as low as 5 per cent.—cases that would be extremely difficult to differentiate on clinical examination without recourse to the somewhat involved "total volume" estimation.

Personally I have on several occasions had cases with colour index nearly unity, but in none of them could the film be mistaken for that of pernicious anæmia, there being 15 per cent. to 27 per cent. of eosinophiles present with very rare normoblasts and no megaloblasts.

Diagnosis is most conveniently and rapidly arrived at by the examination of the stained film. Working with Burroughs Wellcome Leishman tabloids, the making and staining of a film need not occupy more than five to eight minutes, and the examination with a sixth-inch objective of the far end of the film where the leucocytes are thickest, shows in most cases that the eosinophiles are present in considerably increased numbers. In the absence of extensive skin lesions or a history of asthma, the discovery of numerous eosinophiles with no other obvious abnormality should suggest an immediate provisional diagnosis of helminthiasis, probably ankylostomiasis.

In an infected area this is quite sufficient ground for instituting treatment, but in a sporadic case one would confirm the suspicion by examining the fæces when the characteristic ova are perceived in numbers. Fortunately, the ova of the *Necator Americanus*, the New World worm, appear exactly like the ova of the *Ankylostoma Duodenale* under the microscope, except for the slight difference in size ($65\ \mu$ as against $55\ \mu$ for the A.D.), and as the symptoms and treatment of anæmia arising from the two worms are exactly alike, the existence of the two (or more) species need not further concern the clinician. Though our Australian worm has never been properly identified by any authority as far as I can find, I am sure from measurements of the eggs and a study of the adult worms in comparison with some specimens of *Necator Americanus* obtained from Dr. Cole, of Manila, that it is the *Ankylostoma Duodenale*.

Cases are on record in which a few worms have been found *post-mortem* when ova could not during life be demonstrated in the stools, but these instances are uncommon, and apparently an absence of ova has not been noted except with very low numerical worm infection.

Treatment.—Thymol, beta-naphthol and eucalyptus are the most effective drugs. The routine of the books, mag. sulph. at night, followed by 25 grains of thymol on rising, with another 25 an hour later, and mag. sulph. again an hour afterwards, I have found very efficient. This routine was repeated on three successive days, with an intermediate day of non-treatment if the patients were at all depressed by the light diet and the purgation. Four to seven days later if examination of the stools showed any ova (as it occasionally did), the three days' treatment was repeated.

Very young children cannot swallow the cachets in which the thymol is almost of necessity enclosed, and in several such cases I have found eucalyptus effective, though it proved necessary to give the oil, 15 m. in emulsion, three times in the morning on each of three successive days, and repeat the routine two or three times in order to prevent the re-appearance of ova in the stools.

ANÆMIA IN MALARIA.

Examination here reveals erythropenia, colour index unity or a little less, leucopenia, except with a history of recent paroxysms, and an absence of normoblasts.

Craig states that "the pernicious type of anæmia has been met with by the author in six cases, all fatal," in a very large experience. He does not give the colour index, states that poikilocytosis was not marked, and that there were no nucleated reds, and further quotes Bignami and Bastianelli as having also found no normoblasts.

Later on, he says: "In certain instances the classical type of pernicious anæmia may occur as a sequela of the malarial infections, particularly of the aestivo-autumnal variety," but gives no authorities or further particulars. However, he leaves the question more indefinite a few pages further on in his article by writing that malignant malaria leads to anæmia, which is secondary in type, the hæmoglobin diminishing with the fall in erythrocytes (apparently with a colour index slightly less than unity).

At all events, the authors apparently all unite in their observation that in these severe cases there is constant a marked large lymphocytosis which would serve as a distinguishing feature even in the absence of parasites. Personally, I know nothing definite on this point, for in North Queensland the malaria is almost entirely benign in type, and extreme anæmia is uncommon, so that in many hundred films examined I never saw one that could raise a suspicion of pernicious anæmia. But in slides from cases I have been shown, the increase in large hyalines and the black pigment particles in the bodies of the leucocytes, as well as black pigment free in plasma, always stamped the case as malarial.

Blood Volume.—Manson states that the volume of blood is diminished, the evidence adduced being that *post-mortem*, "the venous system, except that appertaining to the spleen liver and portal system generally, may be, markedly empty," and that blood does not flow freely from the pricked finger. But I have been unable to find any reference to estimation of the blood volume by the C O saturation method.

Forms of Malaria.—Malignant infections exist in Northern Territory and many Pacific islands and New Guinea, so that the practitioner may be faced with one of the severe types of malarial anæmia. However, the presence of parasites (and a negative finding cannot be accepted without repeated examinations), or failing this, the large mononuclear increase and the presence of pigmented leucocytes should be sufficient to lead to a diagnosis.

Treatment.—I have not seen many cases of extreme anæmia from malaria with crescents persisting in spite of liberal quinine dosage, but there are such cases. Quinine administered over a long period plus change of climate and careful attention to hygiene and digestion, with iron bæmatinics, does give a satisfactory improvement, but it is much slower in manifesting itself than one would expect, who has seen the rapidity and certainty with which the ordinary benign malarial anæmic is restored to health.

TROPICAL ANÆMIA.

TABLE I.

—	Total blood volume. 100 as standard.	Total Hb.	Oxygen capacity c.c. per 100 grm. body weight.	Total erythrocytes. 100 as standard.	Hb. P.	Erythrocytes per c.c. in millions.	Colour index.	Type of erythrocytes.	Leucocytes per c.c. and varieties.
Normal ...	100	100	·85	100	100	5	1	Normal	7,000 normal
Anæmia from Ankylostoma	194	89	·75	142	45	3	·75	Few normoblasts, some poikilo, and megalocytosis	10,000 to 12,000 eosinophila constant
Malarial Anæmia ...	Decreased	Decreased	Decreased	Decreased	40	2·5	·9	Few poikilocytes	Increased during acute attack, later leucopenia. Large mononucleocytosis
Anæmia from severe Hemorrhage	100	"	·4	"	30	3	·5	Practically normal	Slight increase
Pernicious Anæmia	100	"	·4	"	30	1 to 2	1 ×	Normo and megaloblasts, latter predominate, marked poikilo and megalocytosis	Leucopenia
Chlorosis ...	Increased	100	·8	Increased	40	3	·6	Usually normal	Normal, with decreased eosinophiles

TABLE II.

—	Hb. P.	P. oxygen capacity of blood.	Total blood volume.	C.e. blood per 100 grms. body weight.	C.e. oxygen capacity per 100 grm. body weight.
Normal ...	100	18·5	2,700	9·2	·80
Ankylostomiasis ...	47	8·7	5,520	4·5	·83

[NOTE.—Dr. O'Brien had not an opportunity to revise proofs.—Ed.]

A CASE OF CONGENITAL LEUKÆMIA.

HENRY A. LESCHEN, M.B., M.Ch. Ed., and J. BURTON CLELAND,
M.D., Ch.M., Syd.

Mrs. H., aged 27, an apparently healthy young woman, was on the 10th of July, 1908, confined of a well-grown healthy-looking child. Labour was normal, and I may here add that the mother during the whole period of puerperium never once gave cause for anxiety. The child behaved immediately after birth as an ordinarily healthy child would, but gradually became somnolent and from day to day more hard to waken, while it steadily grew paler, acquiring finally a pale icteric tinge, and weakened until it died on the 15th July, 1908. Nourishment was taken only fairly well, desire for food being at no time marked. It is interesting to know that during the five and a half years of her married life Mrs. H. gave birth to five children, only one of whom, the eldest, a boy, is alive at the present time, and apparently healthy. The second child, a girl, was born thirteen months later, and lived fourteen days; the third, a girl, was born twelve months later, and lived nine days; the fourth, a girl, a seven months child, was still-born twelve months later, and the fifth, a boy, the child here concerned, died on the sixth day. Early during her last pregnancy Mrs. H. consulted me as to her general health, her experience naturally making her somewhat anxious. Her thoracic and abdominal organs were healthy. A blood examination was not made at the time, her general appearance and condition not causing any suspicion of hæmatopoietic trouble. She was given strychnine with small doses of tincture of digitalis, and advised to lead an open air life. About Christmas she left for Victoria and New South Wales, and returned in about three months. On her return to Perth she was taking an iron, strychnine and arsenic preparation. I advised her to persevere with this, and must say that she did well during the rest of her pregnancy. Only quite towards the end was there a little œdema about the feet and hands, which disappeared when lying down, and at no time caused trouble or discomfort, thus differing markedly from her condition in her preceding pregnancy, when œdema was said to have been intense, affecting even the external genitals. At no time was albumen found in her urine. Her confinements, I was told, were generally good, the second labour only necessitating the use of forceps.

Her general health both before and after marriage had been good. She was supposed to have had a touch of anæmia at eighteen years of age, but this, she states, caused no inconvenience.

The prominent and eminently interesting fact of her married life is the giving birth to four children, three of whom died at the ages of fourteen days, nine days, and six days, all very probably from the same cause. The fourth having been premature and still-born, may not count, but probably suffered, or would have suffered in the same way, as may be reasonably inferred from the history and the blood examination of the mother and the one living child.

I just stated that death was probably due to the same cause in three children, and I am led to believe so from the fact that the father informed me that these children had all died in exactly the same way, showing exactly the same symptoms: somnolence, difficulty of wakening, indifference to food, increasing pallor becoming gradually somewhat icteric, increasing debility and death. He went so far as to say that he would be

able to tell on the second day whether the last-born child would live, and I must admit that his experience led him to judge correctly, and he certainly informed me on the second day that the last child was going the way of the preceding ones. He had made up his mind in case the same thing should occur to have a *post-mortem* examination made in order to get at the cause of the trouble. To this end I received full authority to do all that I should think necessary. I may add that Dr. Officer saw the case with me. His opinion was all the more valuable as he had been called in consultation to advise in the case of one of the preceding children. He recognised the conditions as similar in both cases.

When death occurred, Dr. Cleland, assisted by Dr. Cumpston, to whom the whole course of the illness had been carefully explained, was immediately informed, and made a most careful *post-mortem* examination, and the information thus gained was supplemented by a blood examination of the father, the mother, and the only living child, about a month later, and the paternal grand-parents, at a still later date. The family histories on both sides were closely inquired into, but failed to reveal anything definite. Mrs. H. had four sisters and one brother, all perfectly healthy; one brother died four weeks after birth, cause not stated. Her mother died of some throat trouble (cancer?) fourteen days after confinement, and the father is alive and well. One of her sisters has had six children, two of whom died at birth, others are healthy; another sister had two healthy children, one of whom was blind (injury?) and the rest of the sisters have healthy offspring. The brother is not married. Mr. H.'s sisters are all well, and as far as married have healthy children, and his parents are both alive and well. One brother is supposed to have slight heart trouble. Mr. H. and his father faint rather easily, *e.g.*, after a painful blow or knock. Mr. H.'s paternal grandfather died of a fatty heart.

Post-mortem—

Rigor mortis present. Slight yellowish tint of skin.

Lungs.—Not so resilient as normal. Probably not fully expanded.

Heart.—Nothing special, no petechiæ.

Thymus.—Large. Weight, 7.15 grms. (normal weight from birth to two years, 7 to 10 grms).*

Liver.—Dark coloured. Weight, 5½ ozs. (not quite normal weight).

Spleen.—Very large, dark, moderately firm. Weight, 42.5 grms. (normal weight, 9 grms.)

Mesenteric glands.—Slightly enlarged and reddish.

Other lymph glands not appreciably enlarged anywhere.

Reddish coloured marrow in shaft of femur and rib.

Brain.—Nothing special.

Films made from the heart blood show in 200 white cells—

Myelocytes (including eosinophile and transitional cells	...	...	...	37	per cent.
Polymorphonuclear leucocytes	...	...	...	6	per cent.
Lymphocytes, small and large	...	...	...	56.5	per cent.
Nucleated red cells (Normoblasts)	...	...	...	.5	per cent.

To obtain a rough estimate of the proportion during life of white cells to red, the number of each were counted in six fields. There were 406 red cells to 85 white ones—about 5 to 1.

* Dudgeon, Trans. Path. Soc. of London, vol. 55, p. 157.

Films from blood in great vessels of neck show in 200 cells—

Myelocytes	...	...	...	19	per cent.
Polymorphonuclear leucocytes	...	...	...	13.5	per cent.
Lymphocytes	...	...	...	65	per cent.
Large Mononuclears	...	...	...	2.5	per cent.

(Note.—As may be expected in *post-mortem* films, the slides were not very good. In the second count, probably some myelocytes were classified as polymorphonuclear cells and *vice versa*, the cells often being very damaged and hard to distinguish accurately).

Films from femur.—Nucleated red cells.

Films from rib.—Nucleated red cells; eosinophile myelocytes.

Cultures.—On agar, broth, &c.; from liver and spleen, sterile. From heart blood—an abundant growth, apparently a contamination.

Search for Spirochates.—In sections of the liver and spleen, stained by Levaditi's method—none were found.

Microscopical Sections—

Liver.—The capillaries between the liver cells distended with blood in which are myelocytes (some of them eosinophile) and lymphocytes.

Spleen.—Nothing special.

Thymus.—Trabeculæ filled with lymphocytes. Nothing special.

AUGUST 25TH.

Father—

Red cells, 5,360,000 per cmm.

White cells, 8,746 per cmm.

Differential count (200 cells)—

Polymorphonuclear leucocytes	...	73.50	per cent.
Lymphocytes, small and medium	...	22.5	per cent.
Lymphocytes, large	...	1	per cent.
Transitional leucocytes	...	2	per cent.
Eosinophiles	...	1	per cent.

Mother—

Red cells, 4,600,000 per cmm.

White cells, 4,875 per cmm.

Differential count of 200 cells—

Polymorphonuclear leucocytes	...	54.5	per cent.
Lymphocytes, small and medium	...	28	per cent.
Lymphocytes, large	...	10	per cent.
Transitional leucocytes	...	6	per cent.
Eosinophiles	...	1.5	per cent.

Brother, aged 5 years—

Red cells, 4,240,000 per cmm.

White cells, 8,125 per cmm.

Differential count of 200 cells—

Polymorphonuclear leucocytes	...	48	per cent.
Lymphocytes, small and medium	...	45.5	per cent.
Lymphocytes, large	...	1.5	per cent.
Transitional leucocytes	...	1.5	per cent.
Eosinophiles	...	3	per cent.
Large lymphocyte with a few basophile granules	...	0.5	per cent.

SEPTEMBER.

Paternal Grandfather —

Differential leucocyte count (200 cells)—

Polymorphonuclear leucocytes	...	...	55.5	per cent.
Small lymphocytes	...	...	41	per cent.
Large lymphocytes	...	...	1.5	per cent.
Transitional	...	...	1	per cent.
Eosinophile	...	...	.5	per cent.
Basophile	...	...	.5	per cent.

Paternal Grandmother—

Differential leucocyte count (200 cells)—

Polymorphonuclear leucocytes	...	...	52	per cent.
Small lymphocytes	...	...	34	per cent.
Large lymphocytes	...	...	9	per cent.
Transitional	...	...	1	per cent.
Eosinophile	...	...	3	per cent.
Basophile	...	...	1	per cent.

J. BURTON CLELAND,
Government Pathologist (W.A.).

COMMENTS.

This case is of the greatest interest and importance. It appears to be one of congenital leucocythæmia of a mixed myelogenic and lymphathic type—the only congenital case, as far as we are aware, on record. (Le Grand Kerr, in “Diagnostics of the Diseases of Children,” 1907, page 269, says: “The disease is very rare and may be congenital,” but does not quote any authorities).

Differential leucocyte counts have been made of the only surviving brother, the parents, and paternal grandparents. The father's lymphocytes seem to be relatively low, the mother's a little high, the paternal grandparents still higher, and the brother's apparently decidedly above normal. Still, as the mother and the paternal grandparents are none of them related the one to the others, not much stress can be laid on these results.

The occurrence of a case of congenital leucocythæmia is intensely interesting from the point of view of etiology. A child manifesting the disease at birth practically excludes a parasitic theory for the disease, unless some parasite could have entered the fœtus from the maternal side without obviously affecting the mother. This is not, of course, impossible, as is evidenced in congenital syphilis. Nevertheless, the findings in this case strongly support the view that the condition is due to some dyscrasia of certain blood-forming organs.

Professor Muir, in his article on *Leucocythæmia* in Allbutt's System of Medicine, says that “both varieties (*i.e.* spleno-medullary and lymphatic) may affect children a few months or even weeks old.” And, again, “hereditary influences appear to play little or no part in the proclivity to the disease.” Only a few cases are on record in which more than one member of the same family have been affected by the disease. Senator, quoted by Eichorst, observed the disease in twins. Instances in which one member of a family has suffered from leucocythæmia and another from splenic or glandular enlargement are also few in number, and occurrence of leucocythæmia in one of the parents and in one of the children of the same family is almost unknown. Leucocythæmic women bear children free

from the disease, on the other hand, the child of a healthy mother may show the disease when but a few weeks old. Anders, however, in his Text-book of the Practice of Medicine, 8th edition, 1907, p. 476, says that "hereditary influences undoubtedly play a part, a 'lymphogenous diathesis' may thus be transmitted and several generations may be affected by the disease."

A CLINICAL RECORD OF A CASE OF PERNICIOUS ANÆMIA.

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At the British Medical Congress held at Exeter twelve months ago, Dr. Wm. Hunter read an exhaustive paper on Pernicious Anæmia and its allied forms. He reviewed the signs and symptoms at considerable length, and emphasized its remarkable tendency to relapse with almost regular periodicity, and under appropriate treatment the extraordinary manner in which the patient will, for a time, regain good health.

The condition makes its appearance at times with a rapid onset, and although the patient may make an excellent recovery, and, for a time, enjoy apparently perfect health, yet the tendency to relapse is such a characteristic feature of the disease, that the prognosis should always be a guarded one.

I present now the brief notes of a typical case of pernicious anæmia.

The patient, a man aged 58 years, came under observation at the Alfred Hospital at the end of September, 1903. Altogether he was under observation for three years, during which time he had four attacks of profound anæmia, and succumbed to the fourth.

On admission he complained of "dyspepsia," of weakness, and of shortness of breath.

On examination nothing abnormal of any special note was recorded, and after three months' treatment he was discharged.

Some ten months later he was re-admitted, complaining of "utter weakness," loss of appetite, and loss of weight, all of which symptoms had been coming on for four months. On examination there was a general lemon-yellow coloration of the skin and the conjunctivæ even were stained.

The tongue was pale and dry.

The liver edge was palpable midway to the umbilicus.

A blood examination gave—Hæmoglobin, 15 per cent.; reds, 576,000; whites, 3,000.

Under treatment he improved, but his progress was retarded during this attack by developing a mild attack of left basal pneumonia.

He was in hospital altogether for three months.

He was again, for the third time, re-admitted on 20th September, 1905. nine months later, complaining of great weakness and feeling generally "run down," and a recurrence of the dyspepsia—this time accompanied by vomiting of sour fluid, about an hour after food.

On examination the patient looked very ill and wretched, with marked sallowness of the skin and yellowish conjunctivæ.

The tongue was furred and dried, and there was a condition of mild chronic pyorrhœa alveolaris with tartar accumulation about the teeth. No enlargement of the liver was this time detected. The heart's apex beat was in the fifth interspace inside the nipple line, and, on auscultation, there was a definite systolic murmur limited solely to the mitral area.

A blood examination gave almost precisely the same results as on the previous occasion, viz. :—Hæmoglobin, 15 per cent. ; reds, 560,000 ; whites, 2,800.

Notwithstanding the fact that the gastric condition was carefully treated, he steadily lost ground, and on October 2nd, twelve days after admission, I have a note to the effect that he was quite unable to retain anything, and that he was in a very low condition, so much so that later in the day he lapsed into a state of unconsciousness and delirium, and remained in this extremely low condition for the following five days, during which he was nourished by rectal nutrients and saline and brandy enemata alternately.

A blood examination made during this period of great prostration showed no marked change from the previous count, viz. :—Hæmoglobin, 15 per cent. ; reds, 760,000.

On October 7th he regained consciousness, and was able to take peptonized milk, and rectal alimentation was therefore discontinued. The improvement was rapid during the next few days, so much so that he was able to manage a substantial farinaceous diet and a mixture containing iron and arsenic.

On October 16th he developed a mild but troublesome cystitis, which necessitated a daily irrigation of the bladder for a month, before the urine became again clear.

A blood examination taken on October 17th showed—Hæmoglobin, 21 per cent. ; reds, 625,000.

A count made a week later showed a remarkable improvement, viz. :—Hæmoglobin, 25 per cent. ; reds, 1,733,000.

A count taken every few days showed a steady improvement in the blood, until on 4th December, the day before his discharge from the hospital, it reads :—Hæmoglobin, 60 per cent. ; reds, 3,336,000 ; whites, 3,000.

The patient had gained several pounds in weight, and had lost the lemon-yellow tinge of the skin, and now had a ruddy complexion, looked and felt extremely well.

Blood smear examinations were made frequently, and one taken on admission showed poikilocytosis well marked, some microcytes and a few megalocytes. During the time when he was in a very low state smears showed a marked degree of poikilocytosis, megaloblasts were numerous, as many as four being detected in a single field. A few normoblasts and several megalocytes were also observed.

The day before leaving hospital the smear showed the reds much more uniform in size and very little irregularity in shape. No nucleated reds and no megalocytes were detected.

About seven months later—18th June, 1906—he was re-admitted for the fourth time. He stated that he had been in good health until about three weeks ago, when he noticed a rapidly progressive weakness and loss of appetite. The weakness was so pronounced that he had a marked disinclination for even the slightest exertion. Sallowiness of the skin developed rapidly, and on admission it was marked. A slight cystitis was present, but improved with treatment.

A week after his re-admission symptoms of his old gastric trouble developed—beginning with a marked distaste for food, and soon followed by an intractable and uncontrollable vomiting. This time, although his condition was not apparently so bad as on a previous occasion, he showed no response to treatment, and died on 21st July, 1906.

A blood examination on admission gave a count of—Hæmoglobin, 20 per cent.; reds, 490,000.

Ten days later the reds had increased to a million, but the hæmoglobin still remained at 20 per cent.

An autopsy was made by Dr. Mackeddie, who was much struck with the apparently bloodless condition and great emaciation of the body.

The lungs had extensive apical adhesions on both sides, but no obvious tubercular lesion was present. The lungs generally were leathery and functionless.

The heart was flaccid and resistless, with only a poor attempt at hypertrophy of the left ventricle. The cut edge of the muscle showed marked alteration in colour and evidence of degeneration. No valvulitis and no swelling of the valves were present. The kidneys were large and pallid, the capsules stripping readily. The liver was colourless and on section grey and fatty. Both the liver and kidneys were bloodless. The alimentary tract presented no pathological condition.

The management and treatment of the case were similar on each occasion that he was in hospital. The gastritis was treated on general principles, and the patient was given as soon as possible a mixture containing tincture ferri perchloride, to which was added liq. arsenicalis, increased daily by minim doses until seven minims were being taken three times a day. The arsenic was then discontinued for a couple of days, and commenced again, starting with one minim and steadily increased to seven minims three times daily. Bone marrow was also given.

The value of this case lies in the illustration of the relapsing character of the symptoms and signs of one type of pernicious anæmia, and the extraordinary recovery which did take place even when the patient was apparently in a moribund state.

CASES OF ANÆMIA IN THE MELBOURNE HOSPITAL.

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It cannot be claimed that anything like finality has yet been attained in respect to the etiology and classification of anæmias. It is a great advance, however, to know that it is not the blood picture, but the absence of a recognised cause that now differentiates a primary from a secondary. We have come also to recognise the interdependence of bone marrow, spleen and lymphatic glands in the blood making function; that under stress, organs which have ceased to be actively concerned in

adult life may help or replace others, and all may revert to the foetal type in which they once produced both red and white cells. Subject to this mutual replacement, we find that normally in the healthy adult the bone marrow is the source of almost all the red cells and of all the granular white cells; that the lymphocytes are mainly derived from the lymph glands; and that both spleen and prevertebral glands apparently retain a relationship with the red cells, formative as well as destructive. Hence we are prepared to meet, and to some extent understand, variations in the size, shape, staining, nucleation, hæmoglobin percentage, &c., of the red cells; the myelocytosis of increased bone marrow activity, the lymphocytosis of infancy, sepsis, &c., and the leucocytosis which occurs, when in response to toxins, and even physiological stress, the reserve leucocytes pass from the capillaries of lung, liver and marrow into the general circulation.

We thus see how in a mild anæmia the blood may contain immature red cells in proportion to the inactivity of the blood-making organs to meet the call upon them, and how the call may become so urgent that the function reverts to the foetal type, and the marrow undergoes megaloblastic degeneration in respect to both white and red cell formation, whilst the spleen, liver, and glands also take up foetal duties in a way and to an extent proportioned to local conditions and general requirements.

Hospital conditions have not permitted much beyond observation and comparison with pioneer work elsewhere.

My twenty-one years' experience in the Melbourne Hospital may be briefly summarised as follows:—

1. As regards recognised secondary anæmias, we have had numerous illustrations of the usual class of cases dependent upon constitutional and malignant disease, infection, &c., with the customary symptoms and treatment. We have no endemic ankylostomiasis, and only occasional intestinal tapeworms—Beri-beri is occasionally found in our Chinese. In young women we have a large number of cases following removal from country to town, and entry upon domestic service or factory life, and others caused by too long a stay in the continued high temperature of the arid interior—the former ending frequently in gastric ulcer, the latter being very prejudicial to tubercular patients.

2. As regards the so-called primary anæmias, we have a diminishing number of cases of chlorosis, only a few cases of leucocythæmia, splenic, and lymphatic, but an apparently increasing number of cases that are of the type of pernicious anæmia.

Three cases may be quoted in respect to pernicious anæmia. In one, the blood picture was as follows:—

Erythrocytes, 1,700,000.

Hæmoglobin, 40 per cent.

Colour-index, 1.17.

Leucocytes, 4,200.

Films stained with Jenner's solution showed poikilocytosis, megalocytes and microcytes, polychromatophilia, basilar degeneration of erythrocytes, and a few myelocytes and megaloblasts. There appeared to be a relative increase of basilar staining leucocytes, more especially ones of a small mononuclear type, the eosinophiles did not appear to be decreased.

The megaloblasts and myelocytes could not be found in every film.

During life there was a difference of opinion as to whether this was not a profound secondary anæmia; ultimately the course of events was in favour of slowly-progressive cancer of the liver; unfortunately, no *post-mortem* was obtained.

The second was a case that originated apparently in cyanide poisoning and consequent chronic entero-colitis.

A third case has recently been admitted of 8 years' duration, and regarded throughout as pernicious anæmia.

I have seen nine cases of splenic anæmia.

Appended is the family-tree of seven cases of splenic anæmia in one family, six of which are reported at length in *The Lancet*, 8th October, 1904 :—

Grand parents said to be healthy; nothing definite known.	Uncle, æt. 73; very anæmic, history of splenic enlargement 18 years ago.	—	Female, died in infancy.
		—	„ died at 3, from burns.
		—	„ „ 27, from cardiac dropsy.
		—	„ æt. 8, enlarged spleen, splenectomy. Case I.
		—	„ „ 19, „ „ Case II.
		—	„ „ 23, „ no treatment. Case III.
	Mother, æt. 66; healthy. (Father, healthy; died at 46, cardiac dropsy.)	—	Male, healthy, married, one child, no enlargement.
		—	„ „ no enlargement.
		—	„ „ 3 living children, one affected. Case VI.
		—	„ æt. 30, spleen enlarged (Case IV.), 3 children, one affected. Case V.
		—	„ æt. 35, healthy, 3 children living, all healthy.

I have also had under treatment two other cases, both of whom were operated upon (cases VII. and VIII.).

The disease has been variously known as anæmia splenica, primary splenomegaly, the splenic form of pseudoleukæmia, &c., Banti's disease (splenomegalic cirrhosis of the liver) is now regarded as a late stage (*vide* case VIII.). It occurs at all ages in both sexes, is widely distributed, and is apparently the result of some ill-known chronic intoxication. A family incidence has been noted, though not to such an extent as in my series. The clinical picture is characteristic, and includes a slow enlargement of the spleen (later on, of the liver also) with profound secondary anæmia, leucopenia, digestive disturbances, transient icterus, repeated hæmorrhages, pigmentation, attacks of pyrexia, &c., all slowly progressive, but with a remarkable chronicity and variation in severity.

A full report of my series of six cases will be found in *The Lancet*, 8th October, 1904; their subsequent history is contained in this paper. The family incidence was discovered by accident; the second case being noted through visiting the first. It is difficult to say when the anæmia began; even the enlargement of the spleen had existed in some for years unnoticed. The so-called second and third attacks of typhoid fever were probably pyrexial attacks, with already enlarged spleens. The anxious chlorotic look in the severe cases was, and still is, noteworthy.

Case I.—In and out of hospital for some four years—had done no good under the most varied treatment. Since splenectomy (27th September, 1903), she has only required rest owing to her malleolar ulceration breaking down. She has married, suckled her baby, and has ceased to be an invalid.

Case II.—Has remained well since her splenectomy (January, 1904); has married and has suckled twins.

The brother—case IV.—still has his anæmia and enlarged spleen; but may be fairly described as *in statu quo*—practically no change in the five years. His son, with the enlarged spleen, has no symptoms of ill-health, and his spleen is much less, though still enlarged.

Case IV.—The sister—also remains in much the same condition, with a large spleen, and a variable anæmia; but, throughout, she has refused any treatment, examination, or operation.

Thus, though the two who were operated upon are both very much improved, the two adults, who have remained untreated, are still alive, and little, if any, worse than they were five years ago; whilst the boy is practically well, though his spleen is palpably enlarged.

The other two cases are also of great interest.

The following are brief notes:—

Case VII.—M. S. A., æt. 29, good family, also personal history until six years ago, when the left ovary was removed; healthy since; no history of previous anæmia, hæmorrhage, piles, or dyspepsia; no alcohol, no specific attack. A month ago (20th December, 1905) suffered from slight painless jaundice, followed by spasmodic bearable splenic pain, vomiting, with increase in the pain, vomiting daily, vomitus bitter, but free from blood, bowels very free, urine high coloured, slight loss of flesh. On examination—Skin and mucous membranes pale, heart and lungs sound, spleen enlarged to the umbilicus. Blood examination as described. Present condition excellent.

Case VIII.—A. M., æt. 26, admitted 18th March, 1907, family history good; personal, strong as a child, catamenia at 14, regular; married at 25, one child three months old, always lived at Northcote, apparently no previous anæmia. in good health till six years ago, when she was overworked for a long time. Since then has had some twenty attacks of hæmatemesis, some slight, some severe, motions also dark at times, in between attacks has done light work, has gone eighteen months without attack, digestion throughout fairly good. In July, 1901, was treated by Dr. Horne, of Clifton Hill, for an enlarged spleen and ulcerated stomach; in 1902 was seven weeks in the Queen Vic-

toria Hospital, where she was operated upon for (?) ovarian disease, was fairly well till the birth of the baby; a fortnight later had two attacks of hæmatemesis, and a week later two others of considerable amount, followed by ascites; tapped, 15 pints removed. On examination—measured $37\frac{1}{2}$ inches at the umbilicus; tapped 20th March, 1907, 14 pints removed, and again on 1st April, 1907, 10 pints. Spleen fell 2 to 3 inches below the costal margin, liver not palpable, heart and lungs normal. 16th April, 1907, splenectomy; died 30th March, 1907.

Case VII.—Is now perfectly well, so far as appearances and examination go. It is extremely interesting to note that she had "accessory spleens," and that she alone, of all the cases of splenectomy, shows a lymphocytosis. None of the cases have so far shown any noticeable consecutive enlargement of any lymphatic glands.

Case VIII.—One of Banti's disease—was operated upon only because of its otherwise practically hopeless outlook. Statistically, only a slight chance was expected; practically, the case proved even worse than was anticipated. The operator in each case was Dr. R. A. Stirling.

The patent facts from this unusually interesting series seem—

- (a) That splenectomy has been of signal value in three cases of typical splenic anæmia.
- (b) That such patients without their spleens are less susceptible to variations in the number and character of their red corpuscles; lose their leucopenia, may present leucocytosis, and do not present more than the recognised lymphocytosis unless some "accessory spleens" or the like are left behind.
- (c) That a growing boy with splenic anæmia can show a practically normal blood picture, and may be free from any symptoms of disease, even whilst his spleen remains enlarged.
- (d) That cases of splenic anæmia may continue for years unrecognised, may remain stationary even though severe, and not only fail to receive benefit from very varied and continued treatment, but apparently do just as well without it—splenectomy excluded. In this category may be placed the case of the uncle of the three adult patients from the family series. On visiting him at Portarlinton, just before the Congress, I found him an old man, over 70. He was anæmic, with a hæmoglobin percentage, the same as that of Case IV., and with a lymphocyte count of 36.85 per cent. as against 29.34 per cent., and polynuclear 60.85 per cent. against 68.26 per cent. On examination, though no marked enlargement of the spleen could be detected, there was some fullness below the left ribs, and a history was obtained of his having been treated as an out-patient, and also in private, some eighteen years ago, for a general illness and an enlargement of the spleen. Unfortunately, no documentary evidence could be obtained, and Dr. Adams (who had attended him privately) had no recollection of the case.
- (e) That the spleen is evidently not essential to life, digestion, or blood development; that its absence may be satisfactorily compensated in some unrecognised manner; and that its true function still remains to be discovered.

No.	Case and Date.	Sex.	Age.	Erythrocytes.	Leucocytes.	Remarks.
I.	S. H. 5.4.03 to 27.8.03 After Splenectomy— 27.9.03 to 19.5.04 NOW	F.	23	2,350,000 to 3,800,000; slight micro- cytes and polkilocytosis; no polychro- matophilla; colour index, .92 to 1.14. Hæmoglobin, 50-75% 7 times out of 8 well over 4,000,000. Hæmoglobin over 80%. Some polkilocytes	Slight leucopenia up to 8,000; chiefly polynuclear, with a few lymphocytes; no myelocytes or basophiles Slight leucocytosis up to 20,000; occasional myelocyte and mast cell 6,400 { Polynuclear Neutrophils .. 60% Small Lymphocytes .. 29.8% Large " .. 7.53% Eosinophiles .. 2.74% (Nelly)	For details, see <i>Lancet</i> , 8.10.04. Spleen, 43½ ozs. (after being in spirit) General health much improved Married; one child { Red .. 3,070,000 Leucocytes .. 4,800 Hæmoglobin 80% Polynuclear 18.29% Eosinophiles — Small Lym- phocytes 80.15% Large Lym- phocytes 1.14% No oedema, chlorosis, hæmorrhage, fever, or gastro-intestinal trouble. Spleen, 6 x 4 x 2½ inches. Weight, 14½ ozs.
II.	E. H. 9.6.03 to 17.7.03 After Splenectomy— 9.1.04 .. 3.2.04 .. 11.2.04 .. NOW	F.	19	2,780,000 to 3,000,000. Some micro- cytes; slight polkilocytes; a few normoblasts. Hæmoglobin, 50-67%. Colour index, 1 to 1.14 3,700,000. Hæmoglobin, 69% 4,120,000. Hæmoglobin, 80% 4,800,000. Hæmoglobin, 85% Microcytes, 4,160,000. Hæmoglo- bin, 80%	Slight leucopenia, of same type as I. .. Fairly normal. Spleen removed, 19.1.04 .. 6,000 .. 8,000 .. 6,800 { Polynuclear Neutrophils .. 63.77% Small Lymphocytes .. 23.52% Large " .. 8.97% Eosinophiles .. 3.71% (Nelly)	Acute gastro-intestinal attack Free from splenic pain Married, twins; apparently healthy
III.	L. H. 16.5.04 H. H. Uncle, examined now for first time T. H. Brother, now ex- amined for first time	F.	23	Would not permit examination. Hæmoglobin, 70% 4,635,000. Hæmoglobin, 92%	In February, 1903, had twins. Was very anæmic, but placenta not typically specific Polynuclear .. Eosinophiles .. Small Lymphocytes .. Large " .. (Nelly) 6,800 { Polynuclear Neutrophils .. 60.85% Small Lymphocytes .. 1.27% Large " .. 36.85% Eosinophiles .. 1.27% (Nelly) 6,800 { Polynuclear Neutrophils .. 63.19% Small Lymphocytes .. 23.31% Large " .. 5.52% Eosinophiles .. 4.0% (Nelly)	Anæmic and chlorotic for twelve months; no hæmorrhages or gas- tro-intestinal troubles. Spleen below umbilicus and within an inch of the midline. <i>Lancet</i> , 8.10.04. Said to have been in the Homœo- pathic Hospital some eighteen years ago with "a tumour of his spleen." Now some fullness there, but no palpable enlargement Elder brother; healthy, with one healthy child

No.	Case and Date.	Sex.	Age.	Erythrocytes.	Leucocytes.	Remarks.
IV.	A. H. 16.5.04	M.	30	2,250,000. Few micro.; many macro.; no polkilocytes; occasional normoblast. Hæmoglobin, 65%. Staining unequal	9,000 { Polynuclear 80% Lympho and Mononuclear .. 15% Eosinophiles 4.5% No basophiles — Declined operation. Midway in severity between I. and II., whose spleens were removed	Some chlorotic facies; no syncope, vertigo, hæmorrhage, pyrexia, splenic pains, or gastro-intestinal attacks. Spleen notably enlarged
	NOW	..	..	3,785,000. Hæmoglobin, 70% ..	7,800 { Polynuclear Neutrophiles .. 68.26% Small Lymphocytes 29.34% Large 1.73% Eosinophiles .. (Nelly) .. .59%	
V.	R. H. Son of IV. 16.5.04	M.	7	3,600,000. Various sizes micro. and macrocytes; fairly abundant normoblasts; staining irregular. Hæmoglobin, 40%	14,100. Poly., lympho., basic, and eosinophiles all well stained; lymphocytes in excess	Some anæmia; no glandular enlargement; liable to slight pyrexia. Spleen two fingers' breadth below costal margin
	28.5.04	..	..	2,655,000. Normoblasts less numerous	11,200	Apyrexial. Discharged, 5.6.04, much better
	NOW	..	..	4,720,000. Hæmoglobin, 90% ..	6,800 { Polynuclear Neutrophiles .. 64.34% Small Lymphocytes 20.17% Large 4.64% Eosinophiles .. (Nelly) .. 3.1%	Health good; spleen still enlarged
VI.	Niece 16.5.04	F.	10	2,564,000. Great variations in size; numerous normoblasts; diffuse staining. Hæmoglobin, 55%	11,800. Normal types, with definite increase in lymphocytes	Fatter; healthy; anæmic, but not under treatment. Spleen four fingers below ribs. Died subsequently of scarlatina
VII.	Splenectomy, "Accessory Spleens" Mrs. A. 24.1.06 27.1.06	F.	29	1,237,500 2,300,000. Polkilocytes; a few macrocytes; slight polychromatophilia. Hæmoglobin, 45% 3,220,000. Hæmoglobin, 65% .. 4,450,000. Hæmoglobin, 85% .. 1,700,000. Hæmoglobin, 27%. Erythroblasts 3,000,000. Hæmoglobin, 85% 1,600,000. Hæmoglobin, 25% .. 3,700,000. Hæmoglobin, 62% ..	3,600 2,600. Occasional myelocyte 2,600 Marked leucopenia 7,200 No leucocytosis	History given in notes. Spleen movable, and down to umbilicus Under Arsenic and Syr. Ferri. lod. Re-admitted, 20.11.06. Cured for endometritis Splenectomy, 8.1.07. A number of "accessory spleens" noted, but not removed. Liver in early stage of multilobular cirrhosis
	22.2.06 12.3.06 14.7.06 15.8.06 27.11.06 7.1.07					

No.	Case and Date.	Sex.	Age.	Erythrocytes.	Leucocytes.	Remarks.
VII. <i>cont.</i>	<i>After Splenectomy—</i>					
	14.1.07 ..	..	..	1,060,000	10,000, with slight left pleuro-pneumonia	
	16.1.07 ..	..	..	3,900,000. Hæmoglobin, 65%	No leucocytosis	
	6.2.07 ..	..	..	4,650,000. Hæmoglobin, 65%	8,000	
	18.3.08 ..	..	..	1,583,000	5,220	Re-admitted with acute gastro-intestinal attack and slight icterus. Said to have been much better since operation
	9.4.08 ..	..	..	1,800,000. Megalocytes; slight poikilocytosis	3,000	
	5.5.08 ..	..	..	2,680,000. Hæmoglobin, 65%	No leucocytosis	Discharged
	NOW ..	..	..	4,720,000. Hæmoglobin, 80%	(Polynuclear Neutrophils .. 49.24% Small Lymphocytes .. 46.23% Large " .. 3.74% Eosinophiles .. .75%)	Continued well till March, 1908. Re-admitted for gastro-enteritis; in for seven weeks; good recovery. Now in excellent health, feeling ever so much better than before the splenectomy
VIII.	Mrs. M. Case of <i>Ban'tis Disease</i>	F.	26	2,500,000. Slight poikilocytosis. Hæmoglobin, 20%	5,600. Splenectomy; free hæmorrhage from splenic vessels; blood incoagulable. Patient died of free hæmorrhage	For history, <i>vide</i> Notes. Under treatment in 1901 for ulcerated stomach and enlarged spleen; some twenty attacks of hæmorrhage; abdomen thrice tapped for ascites. Spleen enlarged, palpable, and movable

[For all present blood examinations I am indebted to Dr. Nelly; for the remainder to Dr. Mollison.]

APLASTIC ANÆMIA.

G. E. RENNIE, M.D. LONDON, (Sydney).

The President referred to two cases of aplastic anæmia which had recently been under his care in the Royal Prince Alfred Hospital, Sydney. The first was that of a man about 50 years of age, who had previously been strong and healthy. Some two months previous to admission, he had begun to complain of vague pains about the thighs and legs; at the same time he became rapidly weak and anæmic, and was vomiting at odd times without any apparent cause. On admission to hospital his anæmia was seen to be profound, the red blood cells were under 150,005,000, with no megaloblasts and some poikilocytosis; there were also about 20,000 leucocytes. He had no temperature, his spleen was slightly enlarged, but no other enlarged glands. His vomiting persisted, and in spite of all treatment he gradually sank and died in a few days after admission. At the autopsy the organs were generally healthy, except the spleen, which showed a slight geneal enlargement. The bone marrow was intensely pale, and on microscopic examination there were signs of a reaction which had failed, and there was an absence of formed elements. The second case was that of a man about the same age, who was admitted in a very weak condition, suffering from vomiting and some hæmatemesis, the diagnosis being malignant disease of the stomach. Examination failed to reveal any sign of growth about the stomach, and his intensely anæmic condition suggested rather pernicious anæmia. A blood examination was ordered, but unfortunately this was not made. He died in about three days after admission, and *post-mortem* examination showed no macroscopic disease in any organ, except a condition of profound anæmia. The bone marrow, however, presented exactly the same features as in the first case.

The President remarked that these cases were rare, and a paper by Lvenson, in the *Transactions of the American Association of Physicians*, for 1906, gave the best account of the disease that he knew of. The prominent features of this form of anæmia being a rather rapid downward progress, unstayed by any treatment, with a progressive diminution in the red blood cells, and no appearances of new formation of new red cells. It seemed to be a form of pernicious anæmia, in which the bone marrow failed altogether to respond to the demands made upon it for supply of new corpuscles.

DISCUSSION ON ANÆMIA.

DR. J. F. WILKINSON opened the discussion.—He said he wished to raise the question of "What was Pernicious Anæmia?" He admitted it as a clinical entity, but could not regard it as a primary anæmia. It must be secondary to something. We had always looked on the ankylostomia anæmias as types of pernicious anæmia, but Dr. O'Brien apparently wished to make a distinction, in that there was eosinophilia in these anæmias, but not in pernicious anæmia, and that the colour index was *minus* in ankylostomia anæmia, and *plus* in pernicious anæmia. Dr. Wilkinson had seen cases of pernicious anæmia with distinct eosinophilia, and had failed to find any evidence of helminthiasis after thorough examination of the fæces; and further, he always doubted the accuracy of hæmoglobin estimations in the region of 20 or 25 per cent. He did not believe that any of our ordinary clinical methods of estimating

hæmoglobin were sufficiently accurate to enable us to make such fine distinctions when the readings were low. He did not think that any case should be diagnosed as pernicious anæmia from the blood picture alone; but that if the blood picture suggested pernicious anæmia, careful search should be made, first of all, for possible intestinal worms, and then for any other possible cause of blood destruction. Nitro-benzol poisoning, for instance, and even malignant disease, in which sometimes the blood picture was indistinguishable from pernicious anæmia, and failing the discovery of any cause, the case might then justifiably be labelled pernicious anæmia. He referred to the remarkable variations in the course of the disease, and particularly drew attention to the attacks of coma which sometimes occurred. He had had two cases in which coma was removed from, one of which was comatose for three days, and then woke up and asked for some beef to eat. The importance of this factor was in relation to prognosis, for, unless one was prepared for the recovery, the occurrence of coma might lead to a very serious error in prognosis. But, with all the tendency to spontaneous recovery, relapse occurred sooner or later, and he only knew of one case that had lived more than four years after the disease had been diagnosed. He was very much interested in Dr. O'Brien's remarks that the pallor of the tropics was not due to deficient hæmoglobin value of the blood. He was constantly meeting cases, diagnosed as anæmia, and treated, without benefit, with iron, in which the hæmoglobin content was normal. Most of these cases were constipated girls, and they were often much benefited by treatment with Mag. Sulph.; and he had a shrewd suspicion that when the late Sir Andrew Clark lauded Mag. Sulph. as a specific in anæmia, it was this class of case he was treating. This fact emphasized the need for careful blood examination in such cases.

DR. WILKINSON then showed Barker's new hæmocytometer, in which the cover of the counting chamber is clamped down, and the drop of diluted blood is allowed to run under the cover slip on to the platform by capillary attraction, thus obviating the difficulty of getting just the right-sized drop, as in the old Thoma-Zeiss, and also the possibility of varying depth of the cell by reason of faulty approximations of the cover slip.

DR. H. C. MILLS said that Dr. Springthorpe's cases were open to the criticism of the advisability of removal of the spleen. Is the spleen to be removed in all cases? He would advocate removal in some cases, but not in all cases. Dr. Springthorpe's paper certainly showed transparent honesty. He (Dr. Mills) questioned if some of the cases were not really cases of pernicious anæmia, with enlarged spleen. In regard to pernicious anæmia, he agreed with Dr. Wilkinson that no case should be diagnosed by the blood examination alone. We must see the patient and take all the facts of the history into consideration.

DR. KONRAD HILLER agreed that every case of pernicious anæmia is a secondary anæmia, and related experiments which had been made by injecting various substances into animals, with the result of producing typical pernicious anæmic blood.

DR. SINCLAIR GILLIES said it was a mistake to call pernicious anæmia a primary anæmia. Experiments had shown that toxins injected subcutaneously gave the appearances of secondary anæmia to the blood;

whereas if given intravenously, the appearances were those of pernicious anæmia. As to the value of treatment, he doubted the value of arsenic, but had seemed to get splendid results from salol, and also from acetozone, by the mouth, and by colon injections. He was inclined to think that Dr. Springthorpe's cases were mild cases of pernicious anæmia with enlarged spleen.

DR. CHARLES BAGE said we want experiments to enlighten us as to the causes of pernicious anæmia. He referred to a class of case coming from the tropics, which occasionally occurred here—Sprue—and related his experience of such a case, when he found—*post-mortem*—that large patches of the jejunum and ileum had apparently lost all their mucous membrane. The blood picture in this case was that of a severe pernicious anæmia.

DR. G. E. RENNIE discussed the treatment of pernicious anæmia, and referred especially to the effect of sulphate of magnesia in some cases he had seen. In one case, coma supervened, and the patient, a man, was given large doses of Mag. Sulph., and in a month left the hospital apparently well. He was re-admitted some months later, with a blood picture of pernicious anæmia, was again treated with Mag. Sulph. and again rapidly improved. Shortly after, he got worse, and in spite of Mag. Sulph., Salol, Beta-naphthol, &c., he got worse still, and went home to die. Later on he came back with gall stones. Such a case showed how difficult it is to estimate the effect of treatment in this disease.

SOME STATISTICS IN BLOOD PRESSURE.

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Physician to the Melbourne Hospital, Lecturer on Therapeutics, &c.,
to the Melbourne University.

The following statistical tables represent the results obtained by using Martin's manometer in some 500 consecutive cases in the Melbourne Hospital. Indirectly, also, they present evidence, though incomplete, upon the class of cases under treatment in our metropolitan hospital, the ages of incidence of different diseases, &c.

Martin's manometer, effective and reliable as it is, gives simply a numerical estimate of the amount of force required to prevent the transmission of the brachial pulse wave to the same radial artery. It has no necessary relationship to the real mean or minimum systolic pressure, the state of the circulation elsewhere, the size of the arterial extension, the condition of the blood vessels, local or general, or the source of the force, whether it be cardiac muscularity, arterial elasticity, nervous impulse, or peripheral resistance. Nor does it vary in any constant manner with the pulse-rate or temperature curve. Details are here omitted, but results taken from all the cases showing marked variations in pulse and temperature

are conclusive. *Per se*, therefore, it cannot be relied upon either diagnostically or prognostically unless these and other important considerations are also duly taken into account. Its main use—like the sphygmograph—is rather as an adjunct and a record, a help to the better education of the finger, suggestive, but not conclusive, in cases of doubt. These general considerations are borne out by a careful study of the foregoing series of cases. The pressure is found to be abnormally low in pneumonia, influenza, and typhoid, yet cases with especially low blood pressure have done well without any special attention being paid to it. Nervous factors seem to have no constant relation to it—it may vary remarkably even with meals—patients with an unusually low blood pressure may die under it, others may never be well unless it is at a level (as 180 in one case)—formerly regarded as intrinsically dangerous. Often the record is extremely high—as in a case of dropsy in a young woman, where the heart was really failing because of the strength of the peripheral resistance. Therapeutically, it always requires expert interpretation, not only because of its inherent limitations, but still more because compensation, unknown to the part, is the law of life as concerns the interworking of the different systems and functions, and what this seems called for locally, and even systematically, is often so altered by the instant co-operation of the rest, that what appears locally imperative has already become more or less contra-indicated upon important general grounds.

As in all else, instruments of precision, if they are to meet with special success, require special knowledge and special experience in their interpretation.

SOME STATISTICS IN BLOOD PRESSURE.

Disease.	Sex.	Under 20.	20 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	Over 80.
Influenza ..	M. F.	110, 110, 117 .. 90, 94, 98, 124 ..	120, 124 80, 84, 90, 90, 104, 104, 110, 114, 120 (Psychosis)	84, 110, 132 (Bronchial)				
Pneumonia ..	M.	Max. Min. Av. 120 .. 70 .. 103 130 .. 118 .. 121	Max. Min. Av. 130 .. 110 .. 119 110 .. 104 .. 108 120 .. 110 .. 115	Max. Min. Av. 128 .. 96 .. 108 140 .. 80 .. 96 130 .. 110 .. 116 118 .. 90 .. 102 160 .. 115 .. 129	Max. Min. Av. 130 .. 110 .. 119* *110, 130*	188*	130*	
		90*, 110*, 108, 112, 124, 130	100, 104, 105, 110, 112, 160*	109, 110, 114, 118, 125 84, 90, 104 ..	10, 112*			
	F.	80, 84, 90, 100, 100, 115	90, 90, 90, 90, 90, 98, 100, 104, 110, 110					
Bronchitis, Pleurisy	M.	Max. Min. Av. 120 .. 100 .. 110	110, 120, 140 130* (M.)		110 142			
Empyema ..	F. ..				102* (M.) 100 (F.)		90* (F.)	
Asthma ..	..	..	Max. Min. Av. 120 .. 110 .. 115 (M.)	148 (M.)	..	Max. Min. Av. 150 .. 100 .. 127 110, 129*	Max. Min. Av. 164 .. 120 .. 143 114*	150
Phthisis ..	M. ..	110, 130, 139* ..	Max. Min. Av. 150 .. 95 .. 123 150 .. 120 .. 138	Max. Min. Av. 120 .. 110 .. 116	..	140, 88, 111, 170		
	F.	110 ..	110, 116 80-90, 70, 96, 110-100, 110, 104, 110-100, 120, 90, 122, 120, 100, 114, 90-114*	100, 120				
Hæmoptysis	..	..	Max. Min. Av. 140 .. 130 .. 135					

SOME STATISTICS IN BLOOD PRESSURE—continued.

Disease.	Sex.	Under 20.	20 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	Over 80.
Endocarditis	M.	Max. Min. Av. 110 .. 88 .. 99*	Max. Min. Av. 140 .. 110 .. 127 128 .. 110 .. 116 130 .. 104 .. 112	Max. Min. Av. 140 .. 126 .. 132 100*				
	F.	120-90, 106 (Peri.) 200-130 ..	1-20 106, 110, 180-168	136	Max. Min. Av. 220 .. 100 .. 199	Max. Min. Av. 150 .. 130 .. 135 150 142	Max. Min. Av. 180 .. 146 .. 168 140 .. 98 .. 125	
Rheumatism	M.	98, 100 ..	120, 120, 125, 125	152 ..				
	F.	104, 110, 116, 122	140, 90, 120, 114, 128, 130, 90, 130, 120, 100- 110 (Chorea)	146, 116, 120 ..				
Gout	M.	..	110, 130-110-118, 120	125 ..	Min. Max. Av. 184 .. 152 .. 168 140 .. 120 .. 132		Min. Max. Av. 190 .. 170 .. 183	
	F.	..	120, 130, 150-130, 148-140, 112 (Rh. Arthritis)	104-110, 110 ..	143, 120 Rh. Arthritis 152, 130, 128, 104, 96-118	100-140		
Chronic Nephritis	M.	..	..	195 ..	F. 170 .. 140 .. 240	F. Min. Max. Av. 170 .. 110 .. 140	F. Min. Max. Av. 180 .. 170 .. 175	
	F.	..	..	103, 116, 123, 170, 180, 110* (Uraemia)	190, 174, 190, 180, 80-60, 80-108	126, 150, 185, 180, 166, 140, 150, 150-130	160*	
Alcoholism	M.	..	Max. Min. Av. 130 .. 120 .. 124 95	108, 110, 123 ..	..	Max. Min. Av. 160 .. 135 .. 147 200 .. 100 .. 161	..	153-120
	F.	..	90, 90-100, 104, 108-100, 120, 120, 120-110, 130, 126-130, 128, 180, 204- 160-200, 150 (Ascites)	110, 110-120, 132, 154, 130-150, 106,* 145*	98, 130, 130, 130, 130-140	170-110, 120-150, 140-126, 190- 134		

SOME STATISTICS IN BLOOD PRESSURE—continued.

Disease.	Sex.	Under 20.	20 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	Over 80.
Arterio-Sclerosis	M.	Aneurism	44, 36, (R.) 110, (L.) 106	Max. Min. Av. 170 .. 130 .. 147	Max. Min. Av. 240 .. 186 .. 218			
	F.	Mediastinal Tumour	44, 48 (R.) 100, (L.) 140 R. 140, L. 92 ..	160 .. 120 .. 145 140-150, 170, 176- 186, 160, 148- 140	196 120-150, 130-140- 120	135 195		
Cerebral Hæmorrhage.. (after attack)	M. F.		Max. Min. Av. 130 .. 100 .. 116	Max. Min. Av. 116 .. 90 .. 102 110	100, 110, 170 .. 220 130-150, 126-170, 150, 200-130, 215-224	154-160, 156* .. 190		
Neuroses	F.	110, 120 130 ..	90, 120, 160 .. 112-125	146, 172-130 120				
Neuronimosis	F.	120, 124-92, 140	110, 110, 110-140, Max. Min. Av. 150 .. 120 .. 133	147, 125				
Epilepsy	M.	..	136 .. 100 .. 120 130 .. 120 .. 123 120 .. 100 .. 113					
	F.	118 ..	130 82-100..	130, 132, 104-134 120				
Exophthalmic Goitre ..	M. F.		130, 110-114 140-98-107 (Sp.), 136 (Friederich), 130-100 (Ataxy.) 140 ..	132* 108	190			
Chronic Spinal	M.	..	..	..	..			
Enteric Fever	M.	Max. Min. Av. 106 .. 82 .. 95	119, 123, 118-110- 115	Max. Min. Av. 120 .. 92 .. 108				
	F.	92 110-104, 110 .. 106, 102, 110, 100, 84, 90, 100, 100, 100, 90	107, 120, 112, 121 114 70, 80, 94, 100, 110, 114	108 102 118, 100				

SOME STATISTICS IN BLOOD PRESSURE—continued.

Disease.	Sex.	Under 20.	20 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	Over 80.
Gastro Enteritis	M. F.	110 ..	100, 107, 110, 119 100, 110, 110 ..	80, 140, 148 100, 110	Appendicitis ..	M. (18), 136 ..	(24), 120-110	
Gastric Ulcer	M. F.		110-76-90, 90, 100 110-100, 114-1221	124, 102 (M.) 10, 120, 124-142	220-170	158		
Cholelithiasis	M. F.		130 .. 120, 130	(Intussusception) 130	..			
Diabetes	M.	..	..	..	156, 200			
Malignant	M.	..	..	132* (Melanitic), 95*, 124-160 (H. Glioma of Brain)	..	120*, 120-100-112		
Heart Failure	M.	108*, 125*	Max. Min. Av. 240 .. 156 .. 201	Max. Min. Av. 142 .. 130 .. 135 280 .. 220 .. 245	240* .. 204 .. 118	Max. Min. Av. 175* .. 128 .. 156 180 .. 170 .. 173	Max. Min. Av. 172 .. 128 .. 153 180 .. 170 .. 173	
			192 130 .. 104 .. 112	184* .. 170 .. 176 106 .. 100 .. 101	122*, 130	145, 150	98*, 127*	
	F.	..	130, 117*	120 ..	..	88 ..	130-180	
Senility	M. F.				(Premature) 150	130, 150-180 ..	Max. Min. Av. 172 .. 128 .. 153 110-130, 130, 140, 160, 175	

Miscellaneous.—Spleno-medullary Leukemia (F., *aet.* 28), 100-115. Pernicious Anæmia (F., *aet.* 46), 80. Splenic Anæmia, 80-88. Plumbism (F., *aet.* 70), 110, 14-25 18. Myxœdema (*aet.* 44), 100-110. Cystitis (M., *aet.* 57), 120; (F., 48), 190, (54) 120. Abscess of Lung (F., 20-40), 100-96. Hydatid of Lung (F., 20-40), 70, 110-104. Pancreatic Disease (40-50), 110-86. Meningitis (F., 32), 140-120-130. Insolation (M., 25), 160 (40-50), 130, 110. Malaria (M., 29), 120. Measles (M., 7), 120 (Bronch. Pneumonia). Dementia (M., 65), 140.

Influence of Temperature.—With fall of temperature, the blood pressure rose in 12, fell in 15, and was stationary in 1, out of 28 cases. With fall of temperature, the blood pressure rose in 21, fell in 9, and was stationary in 2, out of 82 cases.

Influence of Pulse Rate.—With lessened rate, the blood pressure rose in 31, fell in 30, and was stationary in 3, out of 64 cases. With increased rate, the blood pressure rose in 136 to 104, whilst T. fell from 103° to 97°·6°, and P. fell from 128 to 68. In a man, *aet.* 24, suffering from Pericarditis, the blood pressure fell from 136 to 150, whilst T. fell from 104° to 97°·5°, and P. fell from 140 to 90. In a man, *aet.* 44, suffering from Pericarditis, the blood pressure rose from 120 to 150, whilst T. fell from 103° to 97°, and P. fell from 128 to 86. In a man, *aet.* 23, suffering from appendicitis, the blood pressure fell from 120 to 116, whilst T. fell from 103° to 97°, and P. fell from 128 to 86. In a man, *aet.* 40, suffering from Pneumonia, the blood pressure fell from 110 to 94, whilst T. fell from 106°·4° to 103°·2°, and P. fell from 130 to 96. In a man, *aet.* 60, suffering from injury to the head, the blood pressure was at its minimum when T. and P. were at their maxima.

MODERN METHODS IN THE INVESTIGATION OF GASTRIC DISORDERS.

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It cannot be gainsaid that much valuable information, both for diagnosis and treatment may be obtained by properly performed modern methods of investigation in gastric disorders ; but I am afraid these methods are little used, partly from an idea that they are too disagreeable to the patient, and partly that the investigation is beyond the scope of the busy practitioner. In my opinion, both these conclusions are fallacious. I have not found any difficulty in dealing with patients once they realize that increased accuracy of knowledge of their complaint may thus be obtained ; and I hope to show in this paper that the work involved in the investigation is such as any practitioner can easily carry out without any very elaborate apparatus or deep chemical knowledge. As a rule, there are only three things we require to ascertain—

- (a) The secretory functioning of the stomach,
- (b) Its motor functions,
- (c) Its size and position in the abdomen,

and the requisite knowledge can usually be got by one use of the tube. There may be cases in which further investigation will be required, but these are unusual, and may be left to the specialist to deal with. I consider the best kind of tube to be the soft-rubber Van Valzah tube, with solid end, and only one large opening, close to the end, with well-bevelled edges, and a small round opening opposite this to lessen the risk of sucker action. With this, there is very little risk of injury to the mucus membrane, and it is easy to bury the opening in the pool of gastric contents, and so start syphon action. Tubes with open end and one or two large openings are much less effective and more dangerous. It is not necessary to use cocaine to lessen throat irritability. If the throat tends to give trouble, I make the patient take a few deep breaths, and then at once slip the tube past the pharynx. Keep the head well forward, and get the patient to swallow as soon as the tube is over the tongue ; then rapidly push it on, using both hands ; and if there is any choking, peremptorily order him to take a deep breath, and, as a rule, all further difficulty vanishes. I always lubricate with milk.

For extraction of contents, I use Turck's exhaust bottle, which sets up flow as soon as the point of the tube is buried in the pool of contents. If it fails at first, be sure the point is well into the stomach, and then draw it out, inch by inch, till flow begins. If this special apparatus is not available, there is seldom any difficulty in starting the flow by tickling the stomach and inducing retching ; but this is much less pleasant to the patient. On no account must water be put down the tube to start syphon action, as this will give an unknown dilution of the gastric contents, and renders all accuracy of calculation impossible. I find it necessary to emphasize this warning, as I have several times been asked to examine specimens obtained in this way.

Should it be for any reason impossible or inadvisable to pass a stomach tube, sufficient contents may be safely obtained by the use of Einhorn's silver bucket or Turck's capsules, of which I show these specimens. In a case of suspected malignancy, I was able to show absence of HCl., presence of blood, and the Oppler-Boas bacillus by means of a Turck's capsule, which the patient, an old man, found no difficulty in swallowing.



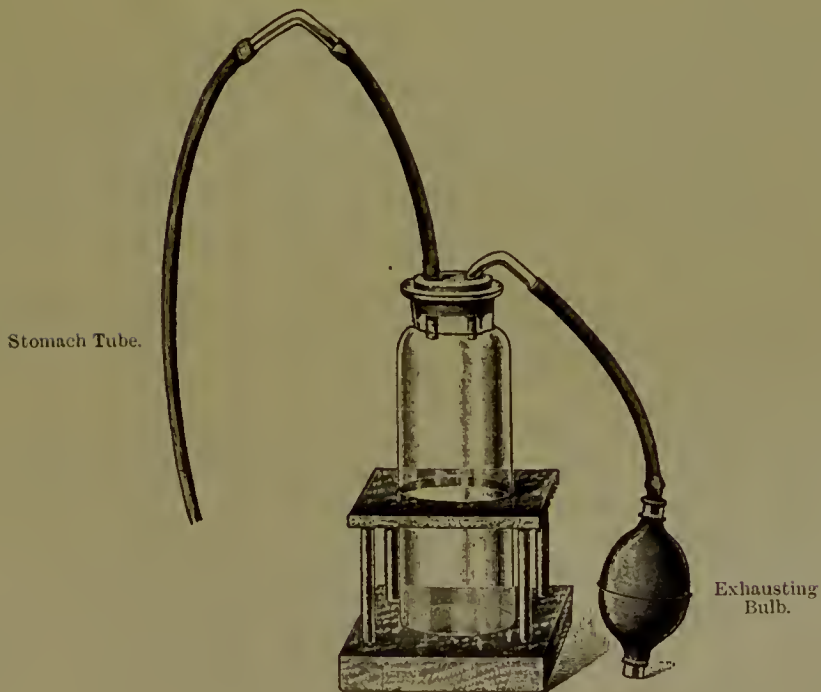
Turk's Capsule closed.



Turk's Capsule after Solution of the Gelatinous Envelope.



Einhorn's Stomach Bucket.



Stomach Tube.

Exhausting Bulb.

Turk's Aspiring Bottle.

I have found that the simpler the test meals used the better, and, practically, I scarcely ever use any other than the double test meal as advocated by Hemmeter. At 7 a.m. I order "a small piece of grilled steak, without fat or gristle—to be cut small and well masticated—two pieces of dry toast, and two cups of weak tea. No other food or drink to be taken till 1 p.m.; then the Boas test breakfast is given—3 oz. stale bread or roll, and 12 oz. water. This to be well masticated and taken during fifteen minutes. Extract it one hour later (2 p.m.). I much prefer bread and water to toast and weak tea, as sometimes advocated; you get a clear result, and can more readily detect anything abnormal, especially slight hæmorrhages or small pieces of meat or growth. In this way we get a fair idea of the motor power of the stomach, for if any of the morning meal is still present at 2 p.m., it is evident there is delay in the passing on of the food, and the matter can be further investigated if deemed necessary.

For instance, should there be a question as to whether the delay is due to narrowing of the pylorus, or to simple myasthenia, we may give the patient a piece of cork, 1 c.c. in diameter, and keeping him in bed, lying slightly over to the left side, the cork will float up to the pylorus, and, in the absence of obstruction, will pass on, and may be recovered in the fæces. Should the cork pass readily, the delay of the food is due probably to myasthenia, and this may be tested by making use of the fact that, in the myasthenic stomach, fluids will be delayed in their passage, whereas in the absence of myasthenia they will readily pass through even considerable degrees of stenosis of the pylorus. Give the patient, on a fasting stomach, 300 c.c. of water, and, one and a half hours later, give 100 c.c. of 1 per cent. dextrose solution (the strength of which should be estimated by Pavy's method). Move the patient about to mix the stomach contents, and at once extract some of the fluid, and again estimate its sugar content by Pavy's method.

Should there be any diminution in the sugar percentage, it means that some of the original 300 c.c. of water was still in the stomach, and that is evidence of myasthenia. The amount of fluid that was thus remaining in the stomach at the end of one and a half hours may be estimated by the equation: $100 \div \text{per cent. of sugar solution in the extract} = 100 = \text{amount of original water still in the stomach, e.g., if 1 per cent. dextrose has been taken, and the extract gives a percentage of .5 per cent., then } 100 \div \frac{1}{2} = 100 = 100$ shows that 100 c.c. of the original 300 c.c. was still in the stomach.

Normally, however, as the result of this double test meal, there should be no remains of the morning meal, and only 50 or 60 c.c. of the Boas meal, which should be a whitish, homogeneous mixture, containing a small amount of free HCl., and filtering readily.

In cases where there is good reason to fear gastric cancer, I use the Boas lactic-acid free test meal. This consists of a teaspoonful of rolled oats in a litre of water, boiled down to half its bulk. This should be given in the morning, and the stomach should have been well washed out the previous evening, and also just before giving the meal. In this way, all superficial lactic acid bacilli are got rid of, and the occurrence of lactic acid in the one-hour extract, with deficient HCl., is very suggestive of malignancy.

Having obtained the specimen, a few drops should be *at once* tested for free HCl. by adding a drop or two of a .5 per cent. alcoholic solution of di-methyl-amido-azo-benzol, which gives a cherry-red colour in the presence of free HCl. I strongly advocate this test for free HCl.; it is

simple, requires no heating, and is absolutely reliable. I have never found organic acids present in sufficient concentration to give a red colour, and I doubt if it is possible for them to be so. I look upon the cherry-red colour given by di-methyl-amido-azo-benzol as absolutely diagnostic of the presence of free HCl. in gastric contents. The specimen should now be examined microscopically for pieces of meat, if the double meal has been used, and for anything like pieces of mucus membrane or other foreign substance.

Then take 10 c.c. of the well-shaken mixture, and dilute with water to about 150 c.c., and at once start to filter the remaining part of the specimen. We now estimate—(1) The total acidity, (2) the free HCl., (3) the combined HCl. These I prefer always to express in terms of decinormal solution rather than in percentage of acid, as the variations are at once obvious and marked. I use Töpfer's methods of estimation, and believe them to be adequate and accurate for clinical work. The only quantitative solution required is $\frac{N}{10}$ NaHO (decinormal soda solution), which any good chemist can make.

The titration is simple. A burette is filled with the decinormal soda solution, and we then proceed to estimate—(1) *the total acidity*, by taking 10 c.c. of contents, diluted to about 150 c.c. with distilled water, in a beaker, to which is added three or four drops of a 1 per cent. alcoholic solution of phenol-phthalein. Run into this the $\frac{N}{10}$ NaOH, constantly stirring, till the pink flush remains permanent, and is not deepened by the next succeeding drop of alkali. If we find it takes, say, 8 c.c. of $\frac{N}{10}$ NaOH to thus neutralize the acidity of 10 c.c. of contents, we express the result in terms of 100, and mark down the t.a. as 80. The normal t.a. one hour after the Boas meal should be about 50.

(2) To estimate *free HCl.*, take 10 c.c. of gastric *filtrate* in a white dish, add a few drops of .5 per cent. alcoholic solution of di-methyl-amido-azo-benzol, and run in $\frac{N}{10}$ NaOH till the red colour is just discharged and an orange substituted. The result is expressed as before. If 3 c.c. are used, the free HCl. is 30. Normally, it should be ten to fifteen.

(3) To estimate *combined acid*, a 1 per cent. water solution of sodium alizarin-sulphonate is used, a few drops being added to 10 c.c. of filtrate, and $\frac{N}{10}$ NaOH run in till a violet tinge is permanent. The proper tinge of violet may be ascertained by adding a few drops of alizarin solution to a test tube half filled with a 1 per cent. solution of Na_2CO_3 . The difference between the result of the titration with alizarin and the total acidity is the measure of the c.a., e.g., if t.a. is 80 and alizarin result 50, then c.a. is 30.

Thus we would have total acidity 80, free HCl. 30, combined acid 30, organic acids and acid salts 20.

Such is, in rapid review, the only quantitative work usually required, and it presents no difficulty whatever, the only pieces of special apparatus required being a burette, a 10 c.c. measure, and a 10 c.c. pipette.

The filtrate may now require to be tested for organic acids by well-known methods, and it is often desirable to test its digestive power. This is most conveniently done by the stained-carmine fibrin method, as described by Harley and Goodbody in their book on "The Chemical Investigation of Gastric and Intestinal Diseases."

A microscopic examination of the deposit on the filter paper should always be made, smears being made in the usual way on glass slides, fixed

in the flame and stained by methylene blue. Sarcinæ, yeasts, red-blood cells, and various bacilli may thus be observed, and especial note should be taken of the large Oppler-Boas lactic-acid forming bacillus.

To ascertain the *size and position* of the stomach while the tube is in position, a Politzer bag may be slipped on to its end and the stomach inflated. It is then quite easy to percuss out the upper and lower borders, and to ascertain if dilation or proptosis be present.

This is practically the whole investigation that is usually required. It does not, of course, cover the whole field of gastric investigation, but my object in this paper is to show what can readily be done, and what, being done, will suffice, in most cases, to give all the information that is requisite.

Most of the cases of "indigestion" that present themselves may be divided into two classes—those in which HCl. is in excess, and those in which it is deficient, combined with motor defects in the stomach. As far as my experience goes, very rarely indeed is pepsin absent or even deficient in ordinary cases of indigestion. From this it is obvious that most of the so-called pepsin digestive nostrums sold are absolutely useless, and the money expended on them is so much money wasted. But the determination of the relative HCl. acidity and the motor powers of the stomach give at once a lead as to suitable treatment—a lead which can be obtained in no other way—for the symptoms are often quite misleading, and give no clue as to which condition is present. In some cases undoubtedly, especially in hyperchlorhydria, we can, from the symptoms, tell the condition, and so direct the treatment; but in most cases I am of opinion that we can only obtain an intelligent idea by means of these methods. Especially valuable has the certainty obtained in some cases proved, where the response to treatment has not been very rapid. I have, over and over again, firmly persisted in the lines of treatment first laid down as a result of a gastric analysis, with ultimately complete success, though for a time the patient was inclined to think that very little progress was being made.

Again, we have to deal with cases where organic changes are suspected. In cases of pyloric stenosis the question of malignancy or otherwise has to be considered. I would be loathe to maintain that HCl. is deficient or absent in all malignant stomachs, even when the disease is advanced, but it certainly is in most cases; and, in the absence of strong reason to fear malignancy from the general symptoms, I would say that my experience has been that an apparent stenosis associated with dilatation and fermentation, with excess of HCl., is usually non-malignant. I think the cases in which the HCl. persists, after malignancy has developed, are usually cases in which the original condition has been ulceration. I have notes of three or four such cases, and the history has materially helped to counteract the possibility of error from placing too much reliance on the gastric analysis. On the contrary, absence of HCl. must not be held too closely to mean malignancy. It frequently happens in chronic catarrhal conditions that HCl. is much diminished or absent; but here, again, the clinical history would probably help. The question of early malignancy is, however, a very difficult problem. Even the apparent presence of pieces of growth in the contents may be misleading. I can quote two cases in which pieces which were apparently shreds of mucus membrane were obtained. Skilled pathologists pronounced them to be fragments of malignant growth. Operation was performed, and in neither case was anything even suspiciously malignant found, and subsequent events have disproved the possibility of malignancy. Can we get any help in such cases? I think the best we can do is to consider the lactic acid development.

In any case, where there is reason to suspect malignancy, the stomach should be well washed out overnight and again in the morning, care being taken to leave it quite clean and empty. Then give Boas' lactic acid free test meal, with a little fresh milk. Extract this in one hour, and if HCl. be deficient or absent, and lactic acid be present, and especially if the large Oppler-Boas bacillus be present, I think at least every such case should be explored, for, even if not malignant, there is some mechanical condition present leading to delay of food and development of lactic acid.

I have found this a valuable test. In one case, where malignancy was suspected, I was led to conclude the opposite, and subsequent events have proved the correctness of the opinion. In another case HCl. was absent, Oppler-Boas bacilli and lactic acid were present, and some mucous membrane, which came up in the wash water, was pronounced to be malignant, but operation showed only adhesions limiting the exit of food from the stomach, and the removal of these has greatly improved the patient's health.

I can quote numerous cases of non-malignant pyloric stenosis in which analysis of gastric contents has led to a correct opinion being given, and I am firmly convinced that we have here a valuable adjunct to our clinical knowledge, which, if carefully applied in conjunction with clinical knowledge, will become more and more valuable in diagnosis and in the determination of treatment.

PNEUMOTHORAX—WITH NOTES OF A RECURRENT CASE.

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In the *Australian Medical Journal* for August, 1893, I gave an account of the history of some cases of pneumothorax, with the object of showing that the cause and mode of production, and the kind of onset, are not regularly so plain and easy as might be inferred from the description given in most of the text-books. Since that time I have seen quite a large number of cases, providing examples of probably every mode of origin, at least as regards those taking rise in some kind of intrathoracic condition. The effect has been to confirm me in the opinion, that pneumothorax very frequently arises in such a gradual or obscure fashion that its existence is not, or is hardly suspected, till a physical examination is made. Either the symptoms are not severe, or they seem to be adequately accounted for, by some disease from which the patient was already suffering. As to the symptoms themselves, and the regularity of their occurrence, it has also to be said that the whole range of them must not be expected to exist in each individual case. The craving for so-called pathognomonic signs, as supplying an easy guide to diagnosis, is natural, on the part of the student or the inexperienced practitioner. But, as a matter of fact, in this, as in most other diseases, any such trust is apt to be deceptive. It may be said that almost any sign or symptom may be lacking; and the checking, carefully, of symptom by symptom may be needed for security. About

the character of the percussion note, statements are often made, which I am inclined to be very sceptical about. Osler, in the earlier editions of his *Practice of Medicine*, did not care to go further than to quote Walshe, as authority for the statement that the note may be dull; but goes so far in the last as to declare plainly that, in some cases, "there is dullness," Walshe's own opinion being given that the note may be "Muffled, toneless, and almost dull." That the note may be a kind of flat tympany may be admitted, but that it is actually "dull," is, I think, a distinct exaggeration, recognising of course the dullness which may be due to fluid.

A sign, about which there is curious difference and uncertainty of statement, is "Metallic tinkling." Many of the sounds, heard when auscultating a chest filled with air, have a metallic ring, voice, cough, and adventitious sounds when present. Metallic tinkling has nothing to do with any of these, neither has it any connexion with the coin sound, or bell tympany, the *bruit d'airain* of French writers. It is very seldom heard, and is perhaps doubtful as to its mode of production. It resembles the sound produced by the fall of a bead or small shot on the bottom of a large empty glass jar; and I have heard it produced, quite distinctly, by the fall of a drop on a layer of water, at the bottom of a similar jar or earthenware receptacle. I have little doubt, therefore, that it is actually produced in the pleural cavity in this latter way. The patient has moved, or been jolted, and after he has just settled, a drop falls from the roof of the cavity on the fluid at the lower level or floor. Conditions have to concur for its production—a good-sized air space, with a sufficient fall—a level water surface, and the accidental fall of the drop while the examiner is listening.

Of single signs, which may be definitely diagnostic, the succussion sound, when clearly located in the chest, has very high value—though it is often absent or indistinct—there being implied, for its production, a good-sized air space and also a considerable amount of thin fluid.

Great importance always attaches to the discovery of amphoric breathing, when clearly characterized, *i.e.*, with a kind of metallic echo, and still more definitely pathognomonic is it, when it has some other metallic accompaniment, in cough, crepitation or voice. It, too, is often absent, and it may come and go, in clear relation to varying phases of a case. If it be the case that we cannot even depend on getting a markedly resonant note on percussion, it may well be said that any one of the symptoms and signs of pneumothorax may be lacking. It is certain that there may be no distinct or sudden onset, no sudden or severe access of pain, or similar appearance of oppression or dyspnoea, and the displacement of organs or parts need not be, though it often is, great. And yet it can also be said that, by care in eliciting signs and symptoms, and checking them one by another, the diagnosis can be made a practical certainty in every case.

About prognosis, no very definite statement can be laid down; and chiefly because it depends on the antecedent condition, which may be of a nature to make a fatal result inevitable, or may be such as to allow of a rapid and complete cure. The question of prognosis chiefly arises in cases of tuberculosis of the lung; and according to what is conveyed in some of the text-books, it might seem as if, with such antecedent, the prognosis must always be gloomy. And yet I am satisfied that it need not be so, that the immediately resulting condition need not be severe, and that actual improvement, with absorption of the air may occur. It has been actually proposed to produce an artificial pneumothorax in some

phthisis cases, by introducing sterilized air, with the object of giving the diseased lung temporary physiological rest. It may be true that rupture of a softened patch, or larger vomica, into the pleura, generally is a dangerous complication, either by the direct mechanical pressure caused, or from septic pleurisy set up. It should not be taken for granted, however, that either of these will follow.

As to treatment, the general rule to be laid down is, that active measures of a surgical kind are called for at an early stage, only for the relief of urgent symptoms of oppression.

Where there is suspicion or evidence of rupture into the pleura of a gangrenous patch in the lung, prompt operation gives the only chance of even temporary benefit.

The pneumothorax, secondary to an empyema, which has ruptured into a bronchus, clearly cannot too promptly be dealt with by free evacuation and drainage outwards. These of course are actually rare conditions, and in most case, perhaps, the misfortune is that operation had, for any reason, been delayed too long. That recovery from pneumothorax as such, and even in cases which plainly are, or certainly become tuberculosis, is most clearly shown by the existence of recurrent cases, of which the one forming the text of this paper is a very striking instance.

The patient F. W., a girl aged thirteen, was admitted to the Alfred Hospital, on 7th April, 1904. Four days before, she woke up with a severe pain in the right side, with great breathlessness. Had not suffered from anything of the kind before, or from bad health in any way. Had a shivering, and cough came on, with some kind of dark red expectoration; had slept little since. Her temperature was found to be 104.2 F., pulse 136, respirations 40. Percussion note on the right side was hyper-resonant; breath sounds feeble over greater part of side, but with distant tubular breathing. There was pronounced amphoric breathing, best heard above the nipple; speech had a metallic ring, and coin sound was plainly elicited. The area of liver dullness was reduced, and no succussion sound, or other sign of fluid in the pleura, could be got. She remained in the hospital for about four months, the amphoric breathing coming and going several times, with corresponding increase and diminution in the amount of air in the pleural cavity. She was discharged apparently recovered, and in good general condition.

She was re-admitted on 11th March, 1905, stating that a fortnight before cough had returned, but without pain or expectoration. Except a discharge from the left ear, she had been in good health since leaving the hospital, and had not had night sweats or loss of flesh, till the time when the cough returned. She had distinct signs of pneumothorax, on the right side, but with suspicion of a phthisical condition developing at the apex. Still, she improved greatly under treatment; and after remaining for about seven weeks, she was discharged fairly well. She was admitted once more, on 30th December, 1905, complaining of cough and shortness of breathing, and signs of right pneumothorax, of the most pronounced kind, were found; every sound, voice, cough, or crepitation having a markedly metallic character, bell tympany being very plain. Amphoric breathing of the most characteristic kind, was also present. Now, also, the signs of phthisical implication of both lungs were much more advanced. On one occasion she had hæmoptysis of 15 ounces.

She remained in hospital till her death on 13th June, suffering chiefly from severe attacks of dyspnoea, which were brought on by the slightest exertion. She had a moderate amount of coughing, but cyanosis

became more distinct and constant. For a time amphoric breathing quite disappeared, its place being taken more by moist sounds of all degrees of coarseness. These were heard all over the left lung and markedly over the upper lobe of the right.

On *post-mortem* examination, the right side of the chest was a mere empty cavity, lined everywhere with a thick creamy layer, but with no appreciable amount of pus or other fluid. The lung was completely pushed back into the hollow beside the spine. The moist sounds on the right side were apparently carried over from the left, getting the metallic quality imparted to them in transit. It was a remarkable thing that, during the whole course of the case, in its three attacks, there were never clear signs of accumulation of pleuritic fluid, and the succussion sign was at no time obtained. The absence of empyema was certainly a very notable feature. Tuberculous deposits were widely disseminated.

THE CALORIE REQUIREMENTS IN TYPHOID AND PNEUMONIA.

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Clinical medicine is increasingly concerned with questions of metabolism. Over and above the processes of hydrolysis, synthesis, toxidation, and behind the enzymes, and hormones of the laboratory, it has to remember the mysterious personal molecular control, which is the essence of bioplasm. Invaluable as outside experiments have so frequently shown themselves to be, their influence is mainly suggestive, and the supreme test still remains clinical verification.

Even in health, the individual seems to rise superior to scientific determination, and the laboratory minimum is not necessarily a personal optimum. It may be quite true, as Chittenden has shown, that certain individuals may maintain their nitrogenous equilibrium for months upon 27 to 28 calories per kilo. body weight, and that, as Rubner has demonstrated, the average starvation metabolism during light work approximates 32 calories per kilo, and that the average maintenance requirement on an average mixed diet varies from 35.5 to 36.6 calories (*i.e.*, 2,240, 2,488, and 2,562 calories respectively for a man weighing 70 kilos); yet these figures can apparently be largely departed from, and the individual remain healthy. In disease, the problem is even more complicated. It seems established that the increased proteid metabolism caused by pyrexia is largely reducible by carbo-hydrate injection, but that the increase produced by the attack of toxins cannot be so prevented, though it may be compensated by regenerative processes. Hence neither a high proteid nor a high carbo-hydrate diet can maintain the nitrogenous equilibrium in a severe fever. In the hands of Leyden and Klemperer, diets of 1,732 calories of meat and milk, and of 3,295 calories of milk, lactose, glucose, and alcohol both failed.

The actual clinical dietary of fever has been evolved, however, not from laboratory experiments, but from clinical experience. In typhoid fever there is the special difficulty that the lesion is situated in the digestive

tract, where functional activity may provoke hæmorrhage, and perforation. Hence the selection of a specially assimilable form of food, in small quantities at frequent intervals. The English practice of twenty years ago, as exemplified in *Quain's Medicine*, was to give 2.3 pints of milk with 1 to 1½ pints of beef tea, or equivalent. Pepper, similarly representing the American school, considered 1½ to 2 quarts of milk as the proper average quantity, lessened if causing indigestion, curds, or diarrhœa.

None of these quantities, of course, come anywhere near the calorie requirements of healthy metabolism—some 8 lbs. of milk for 2,400 calories—which would be far beyond the average febrile powers of assimilation; and any attempt to provide them would be disastrous. Indeed, I well remember how, twenty years ago, the attempts to get the patients to take anything up to 5 pints was accompanied by distensions, curds, diarrhœa, and a high mortality rate. The tendency, indeed, has been in the other direction, so that most hospital physicians have been gradually going rather in the direction of reducing the quantity to the minimum compatible with sustained resisting power. Thus, McCrae, in *Osler's Modern Medicine*, says that 24 to 36 ounces of modified milk, with the whites of six to twelve eggs, in water, are enough for most patients; that if any change is required, it is generally in the direction of reduction, and that there are good practitioners who give nothing but water during the height of the attack. Quite independently, we have been progressively going in the same direction in the Melbourne Hospital, and, so far as I can judge, with apparently satisfactory results. Personally, I have, further, used a food, the sterilized hopped malt extract, which has the great advantage over milk that it is much more easily digested, does not ferment, cannot produce curds, and may be absorbed without disturbing the physiological rest of the inflamed area. Thus, I have locked up the bowels for a number of days after a hæmorrhage, of 8 pints, and after injecting, 3½ pints of normal saline into the median basilic vein, combated the danger of exhaustion by feeding the patient immediately and continuously upon the malt. Proceeding, warily, in the face of possible perforation, we have, of course, often reduced the quantity even of this most assimilable food. And all the time we were under the belief that our food was, roughly, about the same nutritive value as a similar quantity of milk. Recently, however, I had reason to wonder if this were so, and, to settle the matter, I invoked the aid of Professor Osborne, who has kindly determined for me its absolute calorie value. His investigation shows that whilst milk has a calorie value of 21.74 per oz., the malt has a value only of 14.923. I have thus for years past been feeding all my severe cases, all my cases with hæmorrhage, threatened perforation, in fact, all my cases with any abdominal difficulties, upon one-third less food than I expected. My statistics show as follows:—

1900-1	...	98 cases	...	10 per cent. mortality.
1902-4	...	132 cases	...	9.9 per cent. mortality.
1905-8	...	98 cases	...	6 per cent. mortality.

During the same time, the figures for the rest of the hospital have been—1900-1, 285 cases, 12.6 per cent. mortality; 1902-4, 463 cases, 10.6 per cent. mortality; and 1905-8, 414 cases, with 14 per cent. mortality. As I have stated in a paper on "Recent Melbourne Hospital Typhoid Statistics," I think it practically certain that the main factor in the difference in mortality has been the use of the malt instead of milk in severe cases.

Considering the class of cases treated, it may be assumed that a dietary which shows such a low death rate must be regarded as sufficient. I have, therefore, with the kind assistance of Professor Osborne, obtained the calorie value of the actual diet of a patient, who had extensive and deep ulceration, passed the most and greatest amount of sloughs that I have ever seen, had a number of hæmorrhages, and was febrile for 55 days. I append the details in Table IV. It will thus appear that this exceedingly bad case received, during the sixteen days at which his attack was at its height, only some 623.7 calories of the malt per day, and for the 39 other days, when his diet was purely milk, only some 853.3 calories per diem. The quantities proved sufficient for him, and he convalesced satisfactorily. They fairly well represent our average dietary in severe cases.

I am not aware of any similar estimation. It establishes the fact that during typhoid a patient may be tided over a most severe attack on about a third of the requirements of ordinary metabolism.

In the case of pneumonia, we are dealing with a much simpler problem. The average patient, though dangerously ill for the time, will be apyrexial and on the way to recovery in about a week, and there is no danger to be anticipated from the digestive tract. There is thus less need for maintaining the equilibrium, and less risk from the quantity and quality of the food. Authorities do not therefore give the same minute instructions as in the case of typhoid, but content themselves practically with stating that "nutrition should be kept up to the highest point"—(*Osler's Modern Medicine*, Vol. II., page 544).

As regards actual calorie administration, my experience may be summarized as fairly represented in the following cases:—

1. In a typical and uncomplicated case in an otherwise healthy young adult, as is shown on the appended chart, the crisis was natural, and patient convalesced satisfactorily upon a daily average of 852.5 calories. *Vide* Table II.
2. In a severe case of influenza, complicated by pneumonia (double), and which satisfactorily recovered after 21 days' pyrexia, the average calories per diem amounted to 911.7. *Vide* Table III.

Taking these as average cases, I find that out of my last 186 cases of pneumonia, 34 died, giving a mortality of 19.3 per cent. Eliminating 5 as broncho-pneumonia, and 6 as senile, this leaves 175 cases with 23 deaths, a mortality of 13.1 per cent.

A certain percentage of cases die partly of exhaustion, and in these I propose to increase the calorie value of the dietary in the way recommended by Von Leyden and Klemperer (*Handbuch der Ernährungstherapie* 1904. Bd. II., page 345), viz.—by adding 10 per cent. milk sugar to whole milk, thus raising the calorie value by 410 calories per litre, in which case 2 litres would practically cover the requirements.

It will be seen from the foregoing figures that in practice, we are successfully treating patients with typhoid and pneumonia with a dietary far below the theoretical requirements. No doubt there can be no rigid rule even in these cases, and with greater knowledge will come the greater gain that may make all the difference between life and death to individual cases. It is probable that the need will vary with the kind and activity of the toxin, or germ, and that, as in phthisis, and possibly broncho-pneumonia, superalimentation may be the goal of our effort.

TABLE II.—CALORIE REQUIREMENTS IN CASE OF PNEUMONIA.

Food.	March, 1908.					Total.	Total Calories.	Calories Av. Daily.
	12	13	14	15	16			
Malt	4	..	..	6	..	10	149.23	29.846
Albumen and Lemon	6	..	..	6	..	24	71.25	14.25
Coffee	6	6	18	12	12	54	Vide Milk	706.55
Milk	6	..	..	..	..	6	3532.75	28
Coffee and Egg	6	..	..	..	..	12	140 Eggs	56
Cocoa	..	..	6	14	20	40	280	Vide Egg and Milk
Egg and Milk	..	..	..	6	..	6	Vide Egg and Milk	3.5642
Beef Tea and Egg	..	..	..	..	6	6	17.8212	7.8
Broth	..	..	..	..	6	6	39.	6.48
Malted Milk	..	..	..	..	6	6	32.4	Vide Milk
Milk Tea	..	..	..	..	6	6	..	..
Milk and Lime Water	..	..	..	..	6	6	..	..
Milk and Water	40	..	42	6	..	88	..	..
Milk and Broth	6	..	..	12	6	24	..	..
Milk and Broth	..	..	..	6	..	6	4262	852.5

Daily Average = 852.5 Calories.

TABLE I.

Estimated Calorie Value per Ounce of the
Different Foods.

Malt	..	per oz.	=	14.923
Milk	..	..	=	21.74
Beef Tea	..	..	=	2.97
Cocoa	..	..	=	320
Coffee	..	..	=	Practically Nil
Tea	..	..	=	..
Malted Milk	..	..	=	5.3856
Plasmon Jelly	..	..	=	7.859
Mutton Broth	..	..	=	4.09
Chicken Broth	..	..	=	3.725
Bovril	..	..	=	4.654
Whey	..	..	=	7.7
1 Egg, whole	..	..	=	70.
1 Egg White	..	..	=	17.81

TABLE III.—CASE OF INFLUENZA AND PNEUMONIA.—CALORIE REQUIREMENTS.

Food.	August, 1908.											September, 1908.								Total.			
	19	20	21	22	23	24	25	26	27	28	29	30	31	1	2	3	4	5	6		7	8	
Ozs.																							
Cocoa	12	24	6	6	..	..	12	10	12	12	6	12	..	6	..	12	12	12	6	..	12	172	
Coffee	24	12	..	10	..	..	6	18	18	12	6	10	6	18	..	18	12	6	12	8	6	202	
Milk	12	12	12	18	..	..	..	18	30	18	42	22	12	18	12	28	6	12	4	6	18	300	
Milk and Water	..	10	..	..	..	..	..	..	6	..	..	..	..	..	..	..	..	..	..	..	..	10	
Broth	..	6	..	..	..	..	..	..	..	..	6	..	..	..	..	..	..	6	..	..	..	24	
Milk and Soda	..	..	..	..	..	..	..	..	..	..	..	..	..	6	..	..	..	..	..	..	..	12	
Milk and Egg	..	6	6	..	6	12	..	..	..	..	..	..	6	..	..	..	6	..	..	..	..	36	
Orange Albumen	..	..	6	..	6	..	..	..	..	6	..	6	6	..	18	12	18	..	6	12	6	102	
Milk and Lime Water	..	..	6	..	..	..	..	..	..	..	..	..	..	..	6	6	6	6	..	..	6	18	
Beef Tea and Bovril	..	..	6	..	6	..	..	..	6	6	6	6	..	..	6	6	6	6	6	..	6	72	
Albumen and Lemon	..	..	18	12	24	22	24	6	..	..	..	..	6	..	6	..	6	6	..	..	..	124	
Coffee and Egg	..	..	6	6	6	6	12	..	..	..	..	..	..	..	6	..	..	6	..	..	..	42	
Milk and Plasmon	..	..	12	..	6	6	6	..	..	6	..	12	6	12	..	6	..	12	6	6	6	84	
Coffee and Plasmon	..	..	..	6	6	6	6	6	..	..	..	6	6	6	..	..	..	6	6	..	..	30	
Tea and Milk	..	..	..	10	6	6	6	6	..	..	..	6	6	6	..	..	..	..	..	..	..	46	
Beef Tea and Plasmon	..	..	..	..	..	..	..	..	..	6	..	..	..	6	6	..	6	6	..	..	..	12	
Broth and Plasmon	..	..	..	4	..	..	6	..	..	6	..	..	..	6	6	..	6	6	..	..	..	46	
Tea and Plasmon	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	18	
Cocoa and Plasmon	..	..	..	..	..	6	..	..	..	..	..	..	..	6	..	..	..	..	..	..	..	6	
Broth and Bovril	..	..	..	..	6	6	..	..	..	..	..	..	..	..	6	..	..	..	..	6	6	6	
Beef Tea	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	6	
Broth and Beef Tea	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	6	6	6	
Tea	..	..	..	..	..	..	..	..	..	6	..	..	..	..	..	..	..	6	..	6	..	24	
Total Amounts.																							
748 ozs. Milk	..	..	..	..	..	..	..	16201.52	3 Plasmon Jelly	..	..	..	..	..	..	..	23.577						..
7 Eggs	..	..	..	..	..	..	..	490.	2 Bovril	..	..	..	..	..	..	..	9.308						..
32½ White of Egg	..	..	..	..	..	..	..	578.825	93 Beef Tea	..	..	..	..	..	..	..	276.2286						..
4 Cocoa	..	..	..	..	..	..	..	1280.	58 Broth	..	..	..	..	..	..	..	226.606						..
Total for 21 days										= 19145.9 calories										= 911.7 calories per diem.			

RECENT MELBOURNE HOSPITAL TYPHOID STATISTICS.

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The following are the official statistics of the cases of typhoid fever treated in the Melbourne Hospital since 1898, and the corresponding figures for the metropolitan area during the same period :—

Year.	Hospital Cases.	Number of Deaths.	Metropolitan Cases.	Number of Deaths.
1899	277	51	1,387	143
1900	200	26	784	94
1901	183	21	661	69
1902	194	24	751	72
1903	189	18	622	65
1904	212	20	675	71
1905	104	14	388	41
1906	167	16	651	46
1907	87	9	352	48
1908	154	25	493	50

It is not practicable to work out an accurate case-mortality rate from the latter tables. The "metropolitan-cases" area differs from the "Melbourne and suburbs" area of the Government Statist, for which the deaths are given. Further, the deaths include all deaths from hospitals in the metropolitan area, many of which undoubtedly are derived from extra-metropolitan districts. Both areas, however, are constant, and Dr. Norris, Chairman of the Board of Public Health, to whom I am indebted for the figures, considers that as no abnormal conditions have arisen during the period, a fairly approximate case mortality rate may be obtained.

Comparing my own hospital cases, which I have tabulated in three groups, we have the following :—

Years.	My Cases and Mortality.	Other Hospital Cases and Mortality	Total Hospital Cases and Mortality.	Metropolitan Cases and Mortality.
1900-1 ..	98 .. 10 ⁰ / ₀	285 .. 12 ⁰ / ₆	383 .. 12 ⁰ / ₃	1,445 .. 11 ⁰ / ₃
1902-4 ..	132 .. 9 ⁰ / ₉	463 .. 10 ⁰ / ₆	595 .. 10 ⁰ / ₄	2,048 .. 10
1905-8 ..	98 .. 6	414 .. 14	512 .. 12 ⁰ / ₅	1,884 .. 9 ⁰ / ₈
Total ..	328 .. 8 ⁰ / ₅	1,162 .. 12 ⁰ / ₃	1,490 .. 11 ⁰ / ₆	5,377 .. 10 ⁰ / ₃

I have the following details of my last 108 cases (with a death rate of 7.4), which will give a good idea of the class of cases under treatment during the period :—

Twenty-six were admitted within the first week of their fever, 73 afterwards; in 92, the onset was gradual; in 14, sudden. There was epistaxis in 31, headache in 93, hebitude in 73, insomnia in 24, diarrhoea in 4, spots in 83, enlarged spleen in 67, curds and slime in 58, hæmorrhage in 15, pneumonia in 9, thrombosis in 6, relapses in 15, perforations in 5, and death in 8. Most showed comparative bradycardia, becoming absolute in many during convalescence. The average duration of stay in hospital was 43 days; 40 of the cases became apyrexial by the sixteenth day, and 68 were feverish beyond the sixteenth day; one had 4 relapses. As regards

the Widal reaction, 76 gave a positive, 86 a negative, and 32 a partial reaction to the Widal test, and 21 failed to react after three trials (19 after four), though characteristically typhoid. As regards blood count, out of 50 examined 27 were under 6,000 (8 under 4,000, and 11 under 5,000), and 23 over 6,000; of the latter, all but 8 presented complications, such as rigors, thrombosis, pneumonia, periostitis, perforation, hæmorrhage, &c. The blood pressure was generally 100 or below; in one, as low as 70.

Remarks.—The natural onset of true typhoid is gradual as shown by the Wunderlich rise of temperature, the slow thirst, and the gradual taking to bed. Relapses generally show the Wunderlich rise, and may be diagnosed accordingly. A sudden onset, inexplicable on the grounds of concomitant illness, personal peculiarity, or unusual severity, at times goes with small spleen, extensive implication of the large bowel, unusual hæmorrhage, pyrexia, and absence of brown tongue, signs of some other still insufficiently studied germ invasion. The natural effect of the typho-toxin seems—in order—headache, hebitude, with delirium and insomnia in severe cases and various psychoses in the predisposed. Our unusual proportion of hæmorrhages coincides with our late treatment of cases with a history of dietetic errors and ambulation; our large number of relapses may have a similar origin; whilst nearly all our diarrhoeal cases gave a history of a recent “packet of salts,” solid food during pyrexia, or ambulation. One case incubated in England, several came from other States, and many from distant parts of this State. Patients fed on the sterilized malt-extract instead of milk do not distend, do not pass curds, and do not suffer from distension, unless there is a toxæmic paresis of the bowel muscle or a perforation. “Pea-soup” stools, so far from being characteristic of typhoid, are simply the response to the inflamed bowel to improper milk feeding, and early retention of urine, if not due to previous local lesions, is the result of inattention on the part of the nurse during the stage of hebitude. Diagnostically, we rely far more upon the leucopenia than upon the Widal reaction. The former seems pathognomonic of an uncomplicated attack, omitting other diseases easily recognised by other symptoms. Its absence, in the presence of spots or other definite typhoidal symptoms point to some leucocytotic complication. As regards the Widal reaction, a positive result may be due to previous typhoid, whilst a negative reaction may occur in undoubted cases from insufficient production of antibody, a weak or non-specific culture, or an intestinal attack by some non-typhoidal organism, more or less unknown, and often quite as dangerous as the bacillus of Eberth. The present series corroborate the findings given in a paper in the *Intercolonial Medical Journal*, September-October, 1901. In the same journal, for November, 1904, are the details of a case, which died after ignoring a leucocytosis, and relying upon a typical (but *post-mortem* unreliable) Widal reaction. The same paper gives the details of treatment of a case that bled 8 pints, dying subsequently of pneumonia, and not of the bowel complication. It seems as if with better treatment of pneumonia and improved dieting we can largely control every complication except perforation (5 out of the 8 fatal cases died of perforation, and 1 of influenzial bronchopneumonia. Of the 13 deaths in the 1902-4 series, 3 were moribund on admission, 1 died by misadventure, and 1 of purulent meningitis, due to otitis).

To me, the main interest of this paper lies in the mortality tables. In 1899, there were 277 cases treated in the hospital, with a 15 per cent. mortality, as compared with 271 in 1905-6, with 11 per cent. mortality,

and 241 cases in 1907-8, with 14.1 per cent. mortality. Taken in conjunction with the metropolitan-area statistics, these figures show conclusively the diminution in prevalence which has characterized Melbourne and suburbs since the water-carriage system of sewage disposal was introduced; the improvement in recovery rate which characterized the freer use of hydrotherapy; and the subsequent cessation of improvement in death rate under ordinary hospital treatment. They also show that, for the whole period, my death rate has been two-thirds the rate of the rest of the hospital, and for the past four years less than one-half, and always well under the general metropolitan death rate. The duration, the number of cases, the details of one-third, and the general circumstances, seem to warrant the assumption that the series are fairly comparable. If this be so, to what must we attribute this marked and continued superiority? The cases have the same general hygienic measures, the same absence of drugs, except for symptomatic purposes, and practically the same nursing. The main differences have been the hydrotherapy and the dieting.

(a) My results should finally dispose of the extravagant claims of the cold or other tub baths, for systematic use, in all but the slightest cases. I doubt if all my cases together had a dozen tub baths between them, and yet my mortality for the past four years has been only 6 per cent, and for the past nine years 8.5 per cent. So much for the cold or tub bath "saving five to seven patients in every 100 if systematically carried out," and giving a lower mortality than any other form of treatment (*vide Osler's Modern Medicine*, Vol. II., pages 213-214.) I have always contended (*Australian Medical Journal*, May, 1896, *Intercolonial Medical Journal*, November, 1904), that it is the free use of water at the most appropriate temperature that is the essential point, and I have found free sponging suffice for mild cases, and "gutter baths" equally efficient for severe cases and for emergencies. Further, the gutter bath is easier of general application, potentially less dangerous, and more consistent with that absolute rest which may be of vital importance in bad cases. I would go so far as to say that it can be used when the tub bath cannot, and that, now and again it may save a case that cannot stand the tub bath, or does not get it because it is "contraindicated." And yet, whilst practically insisting on the tub, McCrae (in *Osler's Modern Medicine*) does not even mention the gutter bath.

(b) Much more important, however, in my opinion, has been the difference in dieting. Even since 1894, I have progressively relied upon the sterilized hopped malt extract *vide Australian Medical Journal*, July, 1894), in place of milk in all cases with distension, curds or hæmorrhage, threatened or existent. In 1901 (*Intercolonial Medical Journal*, October), I wrote: "I have used it during the past seven years in over 500 cases, and am satisfied that it is superior to milk in the dietary of typhoid. Indeed, whenever a case is beginning to distend, pass curds, or show signs of abdominal complications, it is my custom to replace milk or peptonized milk by the malt, confident that it will prevent further distension, check the formation of curds, and be absorbed much more easily. It can be used much earlier than milk in the treatment of typhoid hæmorrhage, and as perhaps the main danger of such hæmorrhage is exhaustion, the superiority of the malt is a matter of the greatest importance." I also quoted a case of eight hæmorrhages, of 106 ozs. in 54 hours, in which food was discontinued, the patient dying of exhaustion the following day; another of five hæmorrhages of 46 ozs. in fourteen hours, which recovered after the early use of the malt;

and in the *Intercolonial Medical Journal*. November, 1904, a case of three hæmorrhages of eight pints in three and a half hours, that was placed upon the malt immediately after the third hæmorrhage, and saved (only to die of subsequent pneumonia). Ever since, all my bad cases have been placed upon the malt, with marked improvement in all abdominal symptoms, and the maximum of rest to the inflamed parts, assimilation taking place without any indigesta passing on. In a recent case, which recovered, after passing the largest amount of sloughs that I have ever seen—several large bottles full—the patient was fed during the dangerous period almost entirely upon the malt.

(c) The quantity of food given may have something to do with the favorable results. As will be seen from my paper on "The Calorie Requirements of Typhoid and Pneumonia," I have gradually come down to a daily quantity of malt, or milk, representing simply calories. In this under-feeding—from the old point of view—I find I am but administering much the same dietary as the best recent American authorities, and even approaching, the comparatively "no food treatment of typhoid," with which Dr. Abramowski startled the Adelaide session of Congress. As I then said, however, I do not see why a patient should not be the better for as much nourishment as can be introduced without injurious effects. Certainly, the actual amount taken is less than I had anticipated, but still there is an "optimum" amount which may vary with the individual patient. It is just possible that in some cases, additional proteid, in others additional carbohydrate may prevent an exhaustion, serious, if not fatal. The best plan, surely, is to be guided by the circumstances of the case, in so far as they can be dietetically interpreted, and met.

As a practical outcome to this paper I venture the opinion that if my series of typhoid cases are a fair representation of our general hospital cases, the superiority of the malt over milk as a food in bad cases has been the main factor in saving some 4 per cent. of cases during the past nine years, and some 8 per cent. of cases during the past four, provided that the cases of the last year (with its unusual mortality), were fairly distributed as regards severity, as to which, however, I have no direct evidence.

RATIONAL FEEDING IN TYPHOID.

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On commencing private practice in New Zealand nine years ago, so convinced was I of the efficacy of a modified form of solid feeding in typhoid that at the request of Dr. Mason, then editor of the resuscitated *New Zealand Medical Journal*, I wrote an article for the initial number advocating a more generous diet. At that time it was thought necessary in deference to general opinion to apologize for a departure so "contrary to the preconceived ideas and practice of most medical men." Eight years have passed, and experience has taught me that my opinion was well warranted.

In the South African campaign, where at one time whilst in charge of a sick convoy from the columns of Generals Plumer, Knox, and Remington, with as many as 140 cases of typhoid, I was able to bring them all

safely into Charlestown Hospital. I had further opportunities of witnessing the benefits of a diet unrestricted to milk.

Later, two years spent in residence in London hospitals brought me into touch with past-masters in progressive medicine, who whilst carrying out the routine regimen yet began to modify it to a slight extent in deference to the results obtained by others less conservative in treatment.

Again, two years' work in Egypt, embracing the control of the great hospitals in the El Tor Quarantine Camp in the Sinai Peninsula, not only made me familiar with the practice of the Continental schools, but convinced me that milk is not the only and most suitable food for adults suffering from typhoid, bacillary dysentery, and other diseases of the intestinal tract. Thus when Professor Smith two years ago published his record with solid feeding at the London Hospital, I again, in the *Lancet*, advocated feeding typhoid cases with tripe, the most easily digested of all foods.

Whilst suggesting a departure from milk as the sole diet, I must not be thought to advocate the abolition of milk entirely from the regimen. Until the tongue cleans the patient has no desire for solid food. Therefore, in the early stages milk must be the chief article of diet. The addition of citrate of soda to the milk, however, makes it not only more palatable, but less liable to cause flatus. I have also found, more especially in treating cases of bacillary dysentery with hæmorrhage, that adrenalin added to milk not only relieved the latter complication, but also improved the character of the stools.

The accepted dictum in the treatment of typhoid was "a good nurse and two or three pints of milk a day." Most, if not all, of us were taught to restrict the diet of typhoid patients to fluids, and not to allow any solid food until the temperature had remained normal for a varying period, averaging nine days, according to most teachers. It was gradually recognised that 20 per cent. of cases recovered with, or in spite of, any treatment. Of late years, however, the mortality of typhoid, in keeping with that of other diseases, has become markedly decreased. There has never been any what might be called specific medical treatment. In the main, it has been symptomatic. However, as regards diet, the custom of giving only liquid food seems to have been as unchangeable as the laws of the Medes and Persians.

In America, a soil more congenial for the development of originality in treatment, some pioneers in solid feeding have had results as excellent as those of Barrs. Shattuck (*Boston Medical and Surgical Journal*, 20th December, 1894), has an article upon "Some points in the Modern Treatment of Typhoid Fever," in which he speaks of our inability to estimate the extent of the internal lesion in typhoid fever, and of the fact that in cases of tuberculous ulceration of the bowel we do not limit the diet to liquids. He can see no reason why patients with typhoid fever should not have anything easy of digestion, and leaving no residue which could excite undue peristalsis or irritation of the ulcerated surfaces in the intestine. For two years he had been giving to patients that could take them the following articles: Eggs (raw or very soft boiled), custards, animal broths, and strained gruels. Shattuck's dietary consists of the following articles: Milk, hot or cold, with or without salt, diluted with lime-water, soda-water. Apollinaries, Vichy; peptogenic peptonized milk, cream and water (*i.e.*, less albumen), milk with white of egg, buttermilk, kumiss, milk with tea, coffee, cocoa; soups—beef, veal, chicken, tomato, potato, oyster, mutton, pea, bean, egg, barley; Horlick's and Mellin's food, malted milk,

panopeptone, bovine, somatose; gruels—strained corn-meal, crackers, flour, barley-water, toast-water, albumen-water with lemon-juice; ice-cream; eggs—soft-boiled or raw, egg-nog; finely minced lean meat, scraped beef; the soft part of raw oysters; soft crackers with milk and broth; soft puddings without raisins, blanc-mange, wine-jelly, apple-sauce, and macaroni.

[Dr. Purdy quoted the experience of Packard and Fitz in America.]

My experience in the treatment of typhoid fever was limited up to the time I quit private practice at the end of 1900, since when the intervening years have been spent at home and abroad, ashore and afloat, in military, hospital, and public services, to four cases in Leeds in 1896, two in 1897, fourteen in Blyth and Cambois, Northumberland, in 1898; five in New Zealand in 1899, and two in 1900. My record of 23 cases has been a singularly fortunate one, as I have not had a single fatal result, though relapses have occurred rather frequently—in no less than six cases. In 1898 I adopted a modified form of Barrs' treatment, as far as diet was concerned. As soon as the tongue began to clean (a favorable development, encouraged by a preliminary dose of calomel—3 grains—repeated in 1-10-grain doses every four hours), I gave the patient, in addition to the ordinary milk diet, whipped eggs, custard, beef jelly, and raw meat juice. After a few days, when the tongue is comparatively clean and the temperature on the downward grade, I give tripe, with mashed potatoes and gravy. After that I give white fish, such as soles, and gradually work up to a full diet, with porridge, cocoa, and bacon for breakfast; meat, potatoes, and rice-pudding for dinner; eggs for tea; arrowroot or sago for supper. Of course, this full diet is only in full swing when the temperature is normal. I have found that the patients lose less weight, have a less tardy convalescence, are able to return to work sooner, and generally get stouter after their recovery than they were previous to the attack. I have only seen two deaths from typhoid fever—one in England, due to perforation, and one in New Zealand, which was engrafted on a chronic phthisical condition. Both of these cases were being treated with the stereotyped diet.

Though my experience of private patients, being limited, counts for little, yet I am firmly convinced from observations then and later in hospital, that the best interests of the patient lie in a diet between the two extremes. In typhoid fever one often gets patients who have a craving for some article of diet totally out of the question. I remember a man begging for some salt herring. In his case the craving was easily lulled by placing a pinch of salt on the tongue. Oranges are good right through the illness, care being taken to remove the pips.

If the publication of this paper induces any practitioner to advance beyond that latest concession to conservatism, the use of Benger's Food, and have resort to a rational diet, I am convinced that he will be conscious to himself of the improvement in the results, and that his patients will be less impatient under the restrictions of the regimen imposed.

To anyone who is still wrapped in the swaddling clothes of tradition let me but urge the use of tripe on behalf of the patient suffering from semi-starvation, as is the case of those who are restricted entirely to milk, a suitable food until people cut their milk teeth, but not a complete food even in health after the permanent teeth appear. The fact that elementary text-books tell us it is digested in an hour ought to carry some weight with those who dread the dangers of anything other than a bland liquid trickling over the patches of Peyer.

SOME CLINICAL DISTINCTIONS IN CHRONIC BRIGHT'S DISEASE.

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As a result of the observation of a large number of cases of chronic Bright's disease, I have been able to confirm nearly all Rose Bradford's ideas, and to formulate a working classification of this disease. I think it is valuable to emphasize the types as we see them in practice. The distinctions between the so-called large white kidney and granular kidney are not simply those of morbid anatomy. The incidence of the disease, the clinical history and the complications are all different, and we are quite as justified in separating them on clinical grounds as on those afforded by gross or minute anatomical changes.

It is undoubted that some cases of acute Bright's disease pass on into the chronic variety, and it has been stated that if albuminaria dependent on Bright's disease, 'persists for more than six months, it is at any rate probable that the malady will become permanent and chronic.

Yet it must be admitted that the great bulk of the cases of chronic Bright's disease are chronic from the beginning, and that the patient is unable to date with any degree of accuracy the onset of his illness.

It is important to recognise that some varieties of chronic Bright's disease are unaccompanied by the presence of dropsy at any time in the patient's life.

Though there are difficulties in the classification of chronic Bright's disease both on clinical and on anatomical grounds, three varieties of the malady can be easily distinguished. The first variety corresponds more or less closely with the condition described as the large white kidney, although, as mentioned above, the kidneys are very often not greatly increased in size.

The average age of a series of large white kidney was 28 years.

The earliest indication of the patient's illness is the occurrence of dropsy.

This may first appear in the loose tissue round the eyes, and in slight cases it is especially noticeable before the patient gets up in the morning; the lower eyelid is œdematous and the conjunctiva also.

Generally dropsy also affects the limbs, and a favorite seat for it is the lower part of the back, as far down as the sacrum. The external genitals, too, are very apt to become swollen, and in extreme cases almost the whole body is distended with fluid, so that it has a bloated appearance, while the extreme anæmia which develops itself gives the skin a pale yellow wax-like colour.

So bloated and anæmic is the appearance of the patient that he is sometimes spoken of as having a large white body, as well as large white kidneys.

It may be said that the typical mode of death in such an affection is either the results of accumulation of fluid in the great serous cavities of the chest or (though very rarely) from œdema of the larynx. In many instances the fatal result is caused by œdema of the lungs, pneumonia, acute pleurisy, or pericarditis. Other cases, again, end by uræmia. While in a few failure of the heart from dilatation of the left ventricle is the direct cause of the fatal issue. The disease is of shorter duration than the granular kidney.

In fatal cases the ordinary duration of the disease is under six months. Instances of this disease are not common.

The clinical characteristics of what is described by Dr. Rose Bradford as the second form of chronic Bright's disease, or as the white contracted kidney, are in my experience remarkably constant. It occurs chiefly in young adults, between the ages of 20 and 35 years. The ages in three of my cases were 18, 19 and 10 respectively. In my experience it is somewhat more common in women. The cardio vascular changes are well marked.

The radial blood vessels are usually very much thickened and tortuous, and the blood pressure is very high. The thickening and tortuosity of the blood vessels may be just as marked in a lad of 20, suffering from this disease, as is much more frequently noticed in a man of 50 years of age. The heart is, as a rule, hypertrophied, the apex beat being displaced downwards and outwards, and the impulse is heaving in character.

Anæmia, emaciation, and the peculiar so-called pasty or earthy appearance were well marked in nearly all my cases.

The wasting may be so severe as to be comparable with that of malignant disease, the similarity being more marked owing to the anæmia which is also present.

Deep pigmentation of the skin is of frequent occurrence, and may lead to a confusion with Addison's disease.

Optic neuritis, retinitis, and retinal hæmorrhages were well-marked in most of the cases. In one of them the optic neuritis was so marked that the examining oculist suggested the possibility of a cerebral tumour.

Most of my cases came under my notice for symptoms of uræmia.

One, a girl of 20, died of uræmic coma. A lad of 20 was admitted under my care to the Melbourne Hospital, with hemiplegia, presumably due to cerebral hæmorrhage. This is, I believe, a very unusual complication of white contracted kidney.

Edema does not usually occur except in cases in which the heart becomes dilated, which is a very unusual event in this variety of kidney disease.

The causes of this variety of Bright's disease are not at all evident.

It does not appear to be related in any special way to acute infections such as scarlet fever, nor to gout, lead poisoning, or syphilis.

It occurs in patients at such an early age that mental and physical strain and excesses in meat and alcohol can be excluded. It has no relation to large white kidney, nor to acute Bright's disease.

The third and last variety of chronic Bright's disease to which I shall refer is that commonly included under the heading of granular kidney.

I have found the signs and symptoms of granular kidney most commonly in males between the ages of 40 and 60.

It is not very uncommon between 30 and 40, when symptoms of gout often show themselves for the first time.

There is often a previous history, and there may be signs of gout, lead poisoning, syphilis, and excess in meat and drink, as well as physical and mental strain. In an appreciable number of cases, after very careful inquiry, I could assign no cause for the onset of the disease. This affection occurs frequently in men of heavy build and with large frames. It is remarkable how often they are well-nourished, muscular, and of good colour. When the clinical picture is complete we find the temporal as well as the radial blood vessels very much thickened and tortuous, and the blood pressure is very high, often over 200.

The impulse of the heart is displaced downwards and outwards, and is heaving and laboured.

The most characteristic auscultatory sign, and one which is the direct result of the increased arterial tension, is the loud, ringing, or metallic quality of the second sound as it is heard at the base of the heart or over the carotid artery.

It is generally stated that in these cases the urine is of low Sp. G. There is no doubt, however, that the Sp. G. varies very much from time to time, and is often 1.025 or even higher.

The amount of albumin is usually not great, and it is sometimes entirely absent for days.

In cases of renal cirrhosis, when perhaps the patient has been admitted with cerebral hæmorrhage, we may fail to detect any indication of cardiac hypertrophy, although at the autopsy a day or two later the heart has been found enormously hypertrophied.

We are not surprised, therefore, when we find cases of granular kidney in which there is no clinical evidence of enlargement of the heart, in which the urinary findings are negative, but in which we may rely for the purposes of diagnosis on the thickening and tortuosity of the blood vessels, and the increased blood pressure.

Dr. S. West has done good service in reminding the profession that granular kidney would not be so often overlooked if the importance of the thickening and tortuosity of the radial blood vessels was more generally recognised.

In cases of granular kidney there is no œdema, except as the result of failure of the left ventricle or of an attack of acute or sub-acute nephritis.

YELLOW SANDALWOOD OIL AS A REMEDY FOR ASTHMA AND BRONCHITIS, ACCOMPANIED BY EXCESSIVE BRONCHIAL SECRETION.

HON. J. M. CREED, M.R.C.S., L.R.C.P.,

A short time since, during the winter, I was suffering from bronchitis, attending which was excessive secretion of mucous—purulent phlegm—causing frequent attacks of coughing.

When considerably annoyed by this cough and secretion, I, whilst searching for some terebene, noticed a box of capsules containing fifteen minims each of sandalwood oil, which had been sent to me as a sample by a wholesale druggist. Reasoning by analogy on the basis that capaiva and cubebs were formerly frequently used in similar cases, I determined to try the oil on myself.

The result was that I quickly obtained relief from the excessive bronchial phlegm, the cough lessening in proportion. I had been continually wakened by coughing during the previous night, but on taking two capsules at bedtime I slept through the following one undisturbed by cough, and awoke next morning with my chest free from mucous secretion.

Acting on this experience I have since frequently prescribed the oil in similar cases, at all adult ages to seventy-seven years, with the best results.

Patients who have once used the capsules always ask for them when necessity again arises, preferring them to any other remedy.

There is but one objection, and that is that when the prescription is dispensed, the chemist's unregenerate apprentice sniggers, and is quite sceptical when informed that they are required as a remedy for chest troubles. Thus estimable old ladies of both sexes are liable to lose their characters at the same time that they get rid of their bronchitis.

I prescribe one capsule of fifteen minims every three hours during the day and two at bedtime, as long as may be necessary.

IS THE NON-INGESTION OF FOODS CONTAINING STARCH AND SUGAR NECESSARY FOR THE SUCCESSFUL TREAT- MENT OF DIABETES AND GLYCOSURIA?

HON. J. M. CREED, M.R.C.S., L.R.C.P.

I desire to invite such discussion and criticism as may possibly supply a satisfactory answer to the above question.

At present I have not been able to decide on the evidence available any really final statement as to the pathological conditions which give rise to the presence of sugar (glucose) in the urine. Nor have I been successful in ascertaining definitely what are the symptoms which distinguish true diabetes from what is termed glycosuria.

It would be an incalculable advantage to sufferers could these problems be solved, or even such an advance towards their solution made as would enable practitioners to decide as to the probable origin of the condition setting up the excretion of sugar by the kidneys. No doubt it is to be found in the other excreta when it is really practicable to conduct exhaustive analysis of each one.

It requires, however, an extremely enthusiastic chemist to carry out a series of exhaustive tests of recent fæces. So far, ascertaining the exact quantity present in the urine in a case of glycosuria is considered sufficient, and in this I agree. Analyses of the saliva have been made and sugar has been found; nevertheless, little information on this point is available.

It is well known that brain injuries, especially those affecting the neighbourhood of the fourth ventricle, set up the excretion of sugar in the urine. This in itself is sufficient to raise the suspicion that close observation might discover signs of lessened nerve force in other portions of the nervous system, especially the sympathetic. The alteration of potency in the latter system would, however, be extremely difficult to ascertain.

Physiology goes to show that what is termed glycogenesis is a function of the liver; but that it is either not generally exercised or that morbid change takes place before the blood becomes impregnated with sugar.

Is it not possible that normal nerve power exercises an inhibitory power on the creation of sugar? If this is so, would not the logical treatment of glycosuria, of whatever kind, be the adoption of some means by which lessened nerve force would be restored and the inhibitory power re-established?

I have devoted for a considerable time past much thought to the subject, and have arrived at the conclusion that the non-ingestion of starch and sugar now insisted upon as essential to recovery in cases of glycosuria, including true diabetes, is not only not necessary but is even inadvisable.

In saying this, I fully realize that though I am not, perhaps, guilty of the grave sin of originality, yet I am running counter to general opinion; but I do not fear to invite criticism of my action and of the evidence—not nearly as much as I desire—which I have obtained in support of it.

At intervals, several cases of glycosuria have occurred in my practice, and having exactly put before the patients the details of the treatment invariably recommended, viz., the absolute non-consumption of food containing starch and sugar, I gave them the fullest information with regard to my own opinion as to what I believed to be the best course of treatment to adopt, asked them to decide which they wished to be used for themselves. These cases all occurred in men past middle age, and generally of superior mental calibre. All of them consented to be treated on the lines I had thought out, and all of them made rapid and complete recovery.

My treatment is for the patient to do everything possible to avoid worry and over-fatigue, to take adequate rest and sleep, to avoid the slightest excess in alcohol, to continue in every other particular their ordinary diet, and to take medicines the formula of which does not include any drug which is in any way toxic, but which are most effective in increasing nerve force.

My success with these patients led me, when I discovered that I was myself passing urine containing upwards of 6 per cent. of sugar, to adopt the same treatment in my own case, with the result that in a very short time the amount of glucose gradually lessened, and finally was found to be completely absent.

It is impossible, except in a hospital, in the practitioner's own house, or, as in my instance, in his own case, to obtain such accurate account of the course followed and periodic records of the result of urine analysis, as to justify publication. I therefore did not venture upon it until I had got the evidence of my personal experience.

On 30th July, 1907, finding that I was losing weight and passing an excessive quantity of urine, I examined it for sugar, and found that it contained a very considerable amount. Weighing disclosed a decrease of 14 lbs. from what had been my normal weight for some years. The urine varied from 80 to 100 ounces in the twenty-four hours. There was marked lessening of sexual impulse. I at once adopted the course of treatment I have set forth, especially avoiding any alteration in my ordinary diet.

From 30th July to 2nd September I was unable to arrange for a systematic analysis of my urine to be periodically made, and could therefore only estimate the quantity of glucose present between these dates. This was done by the addition of a definite amount of urine to a proportionate quantity of Fehling's Solution. When comparison was made between the result on 30th July and that on 2nd September, the date on which quantitative analysis was first carried out, it was apparent that on the first day the copper precipitate was quite double that on the latter, when chemical assay showed there was 3.08 per cent. present, the decrease during the month of August having been gradual and constant. On 2nd September the amount of glucose was 3.08 per cent.; on 6th September, 1.38; 11th September, 1.1; 13th September, 0.69; 16th September, 0.33; 18th September, *nil*.

By this time I had gained 10 lbs. of weight, and my urine had materially lessened in quantity.

It is therefore evident that with the avoidance of nerve exhaustion, and with the administration of nerve tonics and nutritives, the non-ingestion of starch and sugar is not essential to recovery from glycosuria, when the accepted symptoms of diabetes mellitus are present.

I have from time to time found that sugar had returned; but I could always find that some nerve strain consequent on either mental or physical exhaustion had preceded the attack. The most marked recurrence happened during the depression following an attack of influenza. I may say that I have never experienced the exhaustion so generally manifest in diabetic patients who are subjected to the starvation consequent on a strict adherence to the generally prescribed diabetic diet.

Encouraged probably by its use in my own case, several patients who had previously strictly kept to a diet excluding starch and sugar have returned to an ordinary diet, with invariably marked improvement of general strength and feeling.

The causation of sugar by brain lesion is a substantial proof that its occurrence in the urine may be the result of lessened nerve force. The case reported to me of a general bank manager, who invariably has an increase in the quantity of sugar excreted on every mail day, is corroborative evidence. I feel confident that careful inquiry will always show that increase in the amount of sugar is traceable to, or at all events is preceded by, some worry or over-work which has lessened the nerve force.

If this is the case, is it not reasonable to believe that the more than semi-starvation which must be consequent on the exclusion of the carbohydrates from the diet will have but little good effect, and will rather tend to increase that loss of nervous energy which I maintain is the most probable cause of the disease.

I shall probably be met by the statement that it has often been found that an increase in the quantity of sugar excreted follows a lapse from the dietary rules laid down for the guidance of a patient. This, I think, may be accounted for by the nervous depression which in most cases will follow the belief that such departure has greatly lessened, if not destroyed, the chances of recovery. There is no emotion so depressing as fear, and therefore nothing so likely to set up the nerve condition which I assert is the chief factor in the excessive excretion of sugar.

In my own case, being desirous of having some standard which would show how heterodox I am, I asked several friends—eminent practitioners—to tell me what diet I ought to adopt, and this they most kindly did. On comparison, I find that during the time that the quantity of sugar in my urine was so rapidly and regularly decreasing, I was habitually consuming everything so strictly forbidden except treacle and ices.

DISCUSSION.

DR. CHAS. BAGE pointed out the danger of cutting off all carbohydrates in many cases, and said we ought to strive to ascertain if the carbohydrates ingested were in excess of those secreted.

DR. G. E. RENNIE said that we all recognise that an absolutely restricted diet is not necessary in every case—but each case must be considered on its merits. He referred to the nerve strain and tension in the incidence of diabetes, and showed the importance of protecting those threatened with the disease from such influences.

SECTION OF SURGERY.

PRESIDENT'S ADDRESS.

SURGICAL TUBERCULOSIS.

By B. POULTON, M.D., B.S. (Melb.), M.R.C.S.,

Lecturer on Surgery, University of Adelaide; Surgeon, Adelaide Hospital.

My first duty is to express my profound sense of the honour conferred upon me by the Committee of Congress in appointing me to this chair, an honour which I am deeply conscious might have been, if I may say so, more fitly bestowed upon another more worthy of distinction.

As one who in some measure assisted in the early Congress movement initiated in Adelaide, as a resident of South Australia, and a humble worker in the surgical world, I recognise that in laying this burden upon me you desire to honour the sister State in my person. I am deeply grateful, and only wish it were in my power to evidence a complete justification of your appointment.

To take up the mantle left by my predecessors is to assume a great responsibility, and one which I feel little fitted to sustain. The duties of office have been made easy and pleasant by the long and strenuous labours of the Secretaries of the Section, whose activities and resourcefulness have been so generally recognised by members, and to whom I feel very much indebted for the careful and orderly arrangement and conduct of the proceedings.

Previous Congresses, by directing special attention to particular subjects, have exercised a potent and beneficial influence of far-reaching extent; have helped materially to illumine certain fields of work, and have indeed done much good by stimulating individual research in many directions. One recalls the late Sir Thomas Fitzgerald's address of 1887; its lively appreciation of Lister's antiseptic method, and its indication in strong, almost prophetic lines, of the now universally adopted aseptic procedure; the admirable exposition of disinfection in surgical practice given by Dr. Camae Wilkinson in 1892, containing, I think, the first full description of modern aseptic technique published in Australia. One remembers the original work and investigation of Maunsell, the valuable contributions to the literature of hydatid disease by Davies, Thomas, Gardner, Professor Stirling, Dr. Verco, and others; and one feels assured that the Section in its present session has already presented much of great interest and value.

To worthily follow such strenuous leaders in surgical work is, I feel, by no means a light employment. Not the least difficult task has been the selection of a subject worthy of this occasion, a subject to which I might venture to direct your attention from the point of view of one, who, whilst engaged in general practice has some special opportunity for the practice of surgery.

The last two decades have witnessed a very remarkable change in the distribution of medical work; for whilst formerly the practitioner was accustomed to refer a large proportion of his more serious surgical cases to hospital surgeons, during these later years there has been shown a growing tendency towards the enlargement of his functions, and the retention of a fuller responsibility.

There has followed a more widely diffused study of surgical problems, an extended experience of operative measures, and a keener interest in

advanced surgery in all its departments. The country practitioner, who formerly devoted himself almost exclusively to general practice, and the surgical operations of emergency, is now in many instances one who carries out extensive operative procedures of the most intricate nature, and conducts for himself the whole treatment of cases, which at no very distant period were referred to colleagues in the more populous centres.

With this impression in my mind it seems not unsuitable to discuss with you some of the tubercular infections which are of interest, as being more especially likely to fall into the domain of surgery, and to emphasize their importance to the community, and to the medical profession.

In the discussion on tuberculosis in Dunedin in 1896, lesions other than those in the lungs came prominently under notice; but there, as elsewhere, the preponderating importance of pulmonary tuberculosis perhaps unduly minimized the value to be attached to the consideration of many other affections produced by the invasion of the bacillus.

The community at large, and the medical profession generally, is fully impressed with the all-pervading prevalence of pythisis pulmonalis, and the air is full of projects for its more efficient treatment; but I am doubtful whether the public, or even the professional mind, is as fully seized with the wide-spreading damage done by the invasion of other organs of the body, with the consequent impairment of function, loss of health, and in many cases of life—in fine, with the injury done by the tubercle bacillus, apart from that wrought in the lungs.

As indicating the comparative incidence of tubercular affections, the following figures bear out my contention:—

In the Adelaide Hospital, during the six years ending in 1907, of 819 cases of tubercular disease treated in the wards, 42.4 per cent. were infections of other organs than the lungs.

During the same period, of 487 deaths occurring in the city of Adelaide from various forms of tuberculosis, 23 per cent. were infections, apart from the thoracic viscera, and of 4,032 deaths registered in the Commonwealth of Australia during the year 1905, as tubercular, over 19 per cent. were due to lesions in other parts of the body.

Considering further that many cases which die of pulmonary disease have been first attacked elsewhere; and have really died from a secondary infection, one realizes that the incidence of surgical tuberculosis is even greater than that to be gathered from death statistics, and that it is a very important factor in influencing the health of the people.

Nor is it by any means certain that adequate value is attached to the infective potentialities of the disease when unaccompanied by manifest pulmonary involvement. It is indisputable that many forms of surgical tuberculosis in their advanced stages may distribute infectious material capable of transmitting any form of tuberculosis, and yet how little attention is directed to this fact, which is apparently ignored in public sanitary regulations.

The necessity of destroying infected sputum is generally acknowledged, and inculcated, but the virulent discharges from bone, joint, and abdominal visceral lesions, which are equally dangerous to health, are viewed with less fear, are not so usually treated as dangerous, nor destroyed with the like care. Again, the very efficient methods of the sanatorium for the segregation and treatment of pulmonary phthisis, even when combined with the institution of asylums for the incurable cases, leaves much work undone, so long as similar sanatoriums and homes remain to be founded for the care of a large class of germ-distributing sufferers, afflicted with other manifestations of tuberculosis; and the attack on the spread of the disease

will certainly be incomplete until similar institutions are available for many of the advanced cases of surgical disease, especially those occurring among the poor and friendless.

I think, too, the time has arrived when fuller and more particular arrangements should be made for the hospital treatment of all tubercular cases. They should be removed from the general wards and specially housed in a separate part of the hospital, where their aggregation would be of advantage for treatment, for collective observation, study, and clinical research. When possible, the ward should be open on all sides, the roof and floor the only fixed parts, and there should be ready access to a garden. In city hospitals, with no available open spaces, the nearest approach to this ideal will perhaps be best attained by placing the wards on the top of the building, with suitable provision for getting the patients, even then, out into the open air.

May I hope that the new hospital so soon to be built in Melbourne, will surmount the difficulties of site, and provide in the centre of the metropolis a set of wards well adapted for the necessary hygienic treatment of all sorts of surgical tuberculosis patients. Such an installation would not only be of immense continuous service to the inmates, but stimulate endeavour in the same direction throughout Australia.

Notification of infectious disease is on its trial in Australia, and not yet of universal application, even for phthisis. For the subjection of this disease it is of undoubted benefit, and personally, I think the method should be expanded, to include, if possible, all cases of declared tubercular infection of any organ. When the Health authorities take cognizance of all cases of consumption, should they not also be advised of all cases where a tuberculous process in any part of the body is likely to disseminate disease, especially in those attended by the discharge of broken-down tubercular material.

It may be suggested, why draw special attention to these tubercular affections in Australia, a land which is so generally, and so rightly, extolled as possessing a climate peculiarly inimical to the free growth of the bacillus? For this reason: that while, if the advantages of the climate are realized and utilized to the utmost, the tuberculous subject is indeed placed in circumstances favourable to recovery; the facts remain that the disease has been and is constantly being introduced by the immigrant; that many parts of the country are already infected, and that while the free air and the sunlight are generally present in profusion, here, as in the Old World, the individual does not fully realize their value, nor use the air and the sunlight as he might.

We all know that tuberculosis often occurs in sparsely populated, sunny, country places; and we also know that the defective housing of the poorer uneducated inhabitants is a potent, perhaps the main factor, rendering this possible. The dark, unventilated hut or cottage, so often found in the open country, comes as a positive shock and surprise to the visitor, and explains, perhaps, to a very considerable extent, the occurrence of disease even in the otherwise healthy arid interior of the continent. A great deal has yet to be learnt by the rural and bush inhabitant of Australia as regards domestic sanitation, and the care to be exercised in preventing the infection of the domicile. In a limited, though a different sense, it is true, that "all houses wherein men have lived and died are haunted houses."

And further, it is necessary that we should take into consideration the fact that bovine tuberculosis exists to a very considerable extent, not only

in the coastal and more densely inhabited districts, but also, though in a lesser degree, in the remote central regions of the country, and that, in addition to the inevitable consumption of infected milk, animals suffering from tubercular lesions are, with a fine disregard of consequences, not infrequently used for food even when known to be affected. There is, apparently, little doubt that many aborigines in South Australia and Queensland have become tubercular after eating the imperfectly cooked flesh of tuberculous oxen.

Recent investigations seem to warrant the statement of Cornet that the bacilli of human and bovine tuberculosis are the same organism modified in their activity by change of host. The English and German Commissions of investigation both agree that the bovine bacillus is found in a considerable percentage of the cultures made from human tuberculosis, and the bovine bacillus so obtained has been shown to cause general tuberculosis in the ox. The majority of recent investigators disagree with Koch, and maintain the practical identity of bovine and human tuberculosis. Not only then may a tuberculous infection be derived by residence in the bush house or hut, perhaps the temporary home of a health-seeking visitor, but it may occur from the ingestion of the products of infected stock.

The ultimate result of many a tuberculous process is largely influenced by the recognition it receives during its early stages, and by the treatment to which it is subjected so soon as it becomes manifest.

Unfortunately, the commencement of tuberculous disease is often so insidious and attended by such trifling modification of function, that it becomes well established before surgical aid is sought; and not even then is a positive diagnosis made so rapidly and so certainly as is desirable, or even necessary.

We have at present no reliable method of blood examination to rely upon in diagnosis. It is not always possible in surgical cases to examine tissue or fluids for the bacillus, to make cultures or inoculation tests; nor are the diagnostic value and safety of tuberculin injections yet fully ascertained, and while valuable information may often be obtained from X-ray pictures of bone and joint lesions, even in them radiography can seldom give more than ancillary aid in defining the condition.

The conjunctival reaction test of Calmette and Wolff-Eisner is now under close investigation and promises to be of great value. My experience of its use is too recent to enable me to determine whether it is always innocuous and to be relied upon implicitly, but it has manifest advantages as compared with Pirquet's cutaneous reaction method or the injection of tuberculin.

While, however, specific diagnostic aids cannot be considered to have arrived at a safe perfection, they may be of distinct value in doubtful cases, and there is every indication that they will be more largely used in the near future, and will so help in the earlier detection of many an otherwise obscure pathological condition.

Tuberculosis is more largely curable than is generally acknowledged, and although wide infection may rapidly supervene unexpectedly, and irreparable local damage is often done before cure is complete, still in many cases treated early, the expectation is favourable.

The care of a localized tubercular lesion includes not only the measures directed to cure the local disease, but those which are required to repair, as far as possible, damage already done to the part and for the restoration of function, and it must be always remembered that the local lesion is often associated with infection of other parts of the body, and that during

its course there may be dissemination by means of the lymph and blood vessels; that the local lesion may kill by local damage, or by causing a wider distribution of disease.

The general principle underlying treatment recognises the power of the organism to destroy the germ, the possibility of the disease being cured by natural processes, and so the necessity in every case for using all means tending to maintain the general health at its highest possible standard, whilst local treatment is adapted for the local conditions.

In all forms of tubercular disease then, a prime consideration is the maintenance of the general health by placing the sufferer in such circumstances as shall best fit the tissues for their combat with the organism and its toxins.

A liberal and nutritious diet is, of course, necessary; residence, if possible, away from populous centres; and in the country, or in town, an open air life both by day and night.

Antiseptic and aseptic methods have exercised a potent and never to be forgotten influence, have rendered possible in some instances the complete removal of the germ-infected focus, in others its partial removal or destruction; and by decreasing the risks of blood poisoning and general infection have been of the most signal service to surgery.

While modern methods and improved technique have favourably influenced the course of many forms of tubercular disease, and have enabled the surgeon to expect better results, there still remain a number of cases in which operation is not required to effect cure, or in which it is impracticable, either from local conditions or from the involvement of the lungs or abdominal viscera, and notwithstanding the better results now obtained where cutting operations are indicated, there exists a strong tendency towards conservatism, and the limitation of operations of a mutilating nature to those cases in which they are absolutely unavoidable.

Many cases so treated with special means carefully directed to secure functional rest of the implicated part, will make steady progress towards a satisfactory recovery; but in others more intensely infected, or perhaps less resistant from feebleness of constitution, the local process may advance; or the disease may not have come under treatment until extensive damage is already apparent, and areas of tubercle are undergoing retrograde change. While even some of these may ultimately recover without active interference, in many, much may be done by the removal of degraded tissue, or even in some cases of the whole part involved, and it is often desirable to operate not only in cases where extensive mischief has already occurred or threatens to increase, but to hasten a recovery which is probable but protracted.

Typical examples of the advances made in treatment since the dawn of the Listerian era are afforded in the very successful results following the complete removal of tuberculous lymphatic glands, such as those found infected in the arm and axilla, by absorption from an inoculated wound of the hand; the relief afforded by the free clearing out of cervical glands, the seat of a progressive tuberculous adenitis—the radical method of dealing with the so-called cold abscess so often found in connexion with disease of bones and joints, by which the ultimate products of an extensive tubercular process are safely removed from the system, without the old danger of a mixed micrococcal infection occurring.

Even in cases such as psoas abscess where it is impossible to make sure of removing all the infected tissues or the whole of the caseating *débris*,

a careful aseptic technique enables one to rid the organism of the major portion with comparative safety, and a very fair prospect of its being able to make a good ultimate recovery.

Abdominal surgery frequently presents striking illustrations of the power of the tissues to eradicate, or at least to inhibit the further growth of the bacillus, as after removal of tubercular ovaries and tubes; and in tubercular peritonitis, where, without removal of any infected tissue, as a consequence of mere exposure by free incision and the evacuation of the ascitic fluid, recovery takes place.

Looking back over twenty-five years of active practice, and after reference to the available statistics, I have endeavoured to form an estimate of the comparative incidence of tubercular cases in surgical practice, and have come to the conclusion that there has been very little alteration in the prevalence of the disease. The numerical increase in hospital cases has been slight in view of the increased population, and the apparent decrease of cases among the better placed classes of the community is probably a consequence of wider distribution among a much enlarged circle of practitioners. The system of compulsory notification of pulmonary tuberculosis has not existed long enough, nor been so universally applied, as to exert all that influence in diminishing the dissemination of disease which may be expected of it in the near future.

The lesions which come most prominently under treatment in South Australia are those of the hip, the knee, elbow, and ankle joints, the cases of spinal caries, and psoas abscess, the cervical and other glandular infections, the rather numerous instances of genito-urinary tuberculosis. Dactylitis and invasions of tendon sheaths are comparatively rare, nor does one often see an instance of lupus. General peritoneal infection comes only occasionally under notice—very frequently bone and joint lesions seem to have followed alleged injury.

A considerable number come to town for treatment, having become infected, many of them, whilst residing in small isolated country settlements, not infrequently situate far in the interior.

As a rule, the nutrition of the patients is fairly good, and one so commonly finds tuberculous disease in persons presenting none of the classical features said to be typical of the so-called strumous diathesis, that very little, if any, importance is attached to the absence of suggestive alteration in general habit of body or physiognomy. The tuberculous individual may, or may not, present evidences of the strumous diathesis, and is more likely to have become infected by common association than to have inherited any special predisposition.

Turning to the individual sites in which tubercular infection is found, the articular osseous systems present by far the most numerous instances, and spinal and hip affections are certainly the most frequent.

In hospital practice, spinal caries coming under early treatment gives, as a rule, satisfactory results, especially when opportunity is afforded for a period of continuous horizontal rest combined with extension as a precedent to the prolonged use of fixation apparatus, such as a plaster jacket. The inflammatory process may often be modified by blistering or cauterization, so that in the early stages, and where pain is experienced, rest in bed is necessary. But the hospital patient more usually comes with marked curvature, and presenting collections of tubercular material demanding evacuation; or, the abscesses having burst, there are sinuses and mixed infection. The unruptured abscess I prefer to treat after the method introduced in England by Professor Barker, and still think that the use of

iodoform emulsion is beneficial as inhibiting the growth of the bacillus or the development and absorption of toxins. But whether any fixed antiseptic be used or not, I think free initial irrigation with a mild antiseptic solution is generally desirable. In these cases, careful removal of so much of the abscess wall as is safe, and of all loose bone, should be effected. Satisfactory closing of the cavity may follow, but the operation has not infrequently to be repeated more than once. And even in these obstinate cases the method compares most favorably with any other which involves the insertion of a drain. Very occasionally has it been found necessary to attack the infected vertebrae from behind, either for the removal of sequestra of the bodies or for laminectomy in disease of the neural arches and pachymeningitis. Where sinuses communicating with the diseased spine are found open on the skin surface, and mixed infection has followed, similar antiseptic measures for the removal of tubercular *débris* and infected granulation tissue meet with a varying but lesser degree of success, and much difficulty is often experienced in opening up and exploring all the infected tracts. Radiography has already given us a means of more definitely localizing areas of affected and necrosed bone, and so facilitating their more accurate removal. Its use, as has been already suggested, for the demonstration of the course and ramifications of sinuses after filling them with an impermeable innocuous metallic oxide suspended in fluid, may well be given a thorough trial.

I have never attempted to correct deformity by forcible extension and fixation, as I consider such treatment entails grave immediate risks, and is usually unwarranted by any reasonable prospect of ultimate benefit.

In hip-joint disease, as seen in Adelaide, remarkably good results are attained in the majority of cases by steady persistence with the usual methods adopted for maintaining muscular and articular rest; and but rarely is there absolute failure even in those in which it is necessary to operate for the removal of collections of pus or sequestra.

I recall the not infrequent early excisions of this joint, which used to be done whenever extensive caries was accompanied by destruction of the joint, displacement of the head, and the infection of the soft parts, and am convinced that many of these operations were unnecessary. I have excised the hip joint in only thirteen cases for all conditions, and find the indications for the operation becoming of increasing rarity. The best terminal result in a doubtful case—ankylosis, with a minimum of shortening and angulation—is less likely to be attained when excision is performed. My excisions of this joint have all been done in adults.

An energetic attack for the removal of necrosed bone and the purification of sinuses is often necessary, and may need to be repeated many times, and it would be better in some cases if a wider and more extensive exposure were made in the first instance if sinuses were cut down upon and cut out so far as is possible, rather than explored and curetted.

In tubercular affections of the knee joint, I think there is often much time wasted in endeavouring to bring about a cure by prolonged fixation, rest, counter irritation, and pressure, and that such methods are too often continued, even when the disease is progressing and not only producing local damage, but interfering gravely with the general health. In this region, the ready accessibility of the joint allows of full exploration, eradication of the synovial membrane and areas of affected bone, without destroying function; and in the advanced conditions, where the cartilages and bone are more extensively involved, a complete excision, though it necessitates bony ankylosis, gives a better result, and more useful limb than that likely to be secured in any other way. Dr. Swift has recently

drawn attention to the very good results he has attained in children by a complete transverse arthrotomy and free exposure of the joint; but arthrectomy is the operation of choice in the child, excision the safest in the adult. Early exposure of the joint and free cutting out of affected parts is generally preferable to an expectant treatment, when the ankle or the tarsal bones and joints are the seat of disease. In this region, I have been impressed with the great liability to involvement of extra articular tissues, especially tendon sheaths, and with the infrequency of cases in which removal of the principal focus of disease leads to a satisfactory result. In disease of the ankle joint, or of the larger tarsal bones of an advanced degree, a Syme or an amputation of the leg well above all infected tissue has been in my practice more satisfactory than partial operations and excisions, and should certainly take preference of all other measures in the debilitated subject, or one giving any evidence of pulmonary affection.

My experience of the shoulder joint is limited to a few advanced cases in adults, in whom I have found the best and most satisfactory results following removal of the head of the bone. Short of excision, one may limit the disease process to a series of changes terminating in quiescence and the preservation of a fixed joint; but, in view of the very great impairment of function entailed by ankylosis, the long period which must elapse before the arm can be used at all, and the risk of exhaustion, such radical treatment has much to commend it.

In the elbow, ankylosis at a right angle is the best termination one may expect in those processes which are arrested under the use of general measures; an early arthrectomy is indicated whenever there are indications of a complete involvement of the cartilages, and the instances in which prompt removal of a limited area of infected bone alone are followed by cure without ankylosis are rare to find. Subperiosteal excision in the adult, with removal of just so much bone as is necessary for eradication with the preservation of function, is an eminently satisfactory operation.

Tuberculous affections of the lymphatic glands, chiefly those of the neck, come very constantly under notice and treatment, and are of grave importance, not only from their local significance, but as implying the possibility of further implication of deeper groups of thoracic and other glands. Their presence is always a menace of wider extension.

Under suitable hygienic surroundings and appropriate treatment, they frequently undergo absorption, but the process may extend beyond the capsule, invade neighbouring tissues, and, in the resulting degeneration, extensive abscesses may lead to wide destruction, much disfigurement, and the occurrence of a mixed infection, with all its risks and consequences.

In the early stages, and so long as there is no evidence of periadenitis and destruction of the capsule, removal is not demanded nor advisable, but where the disease is progressing and adjacent glands are becoming adherent to each other, and the overlying parts, free excision of the whole mass by clean dissection, and without opening up affected tissues, should be effected with all the care and thoroughness which is necessary in other malignant diseases.

Where large abscesses have formed and much overlying skin is involved, it may be necessary to defer excision until a preliminary evacuation of pus, with antiseptic precaution, is followed by a diminution of active inflammation. Both here, and when septic sinuses are present, free and wide removal of all affected tissue should be attempted, rather than partial scraping, and a reliance on antiseptics reserved for those cases where it is impossible of achievement.

It will be generally acknowledged that infections involving the genito-urinary tract frequently present conditions most difficult to control, and in many of them the disease is so widespread when the sufferer seeks relief that palliative treatment alone is applicable. Even when the testicle alone is affected, the onset is so insidious that the early signs may attract no marked attention, particularly when there has been a pre-existing venereal infection, and on the first examination it is common to find definite nodules in the epididymis, and even extension to the testis and cord. Considering, especially, the grave risks of an ascending infection and the practical impossibility of complete extirpation, when once vesicles, prostate or bladder, are involved, I think, in the absence of graver disease elsewhere, no time should be lost in removing epididymis and cord, preserving the testis whenever it is intact, and generally that any other partial operations should be reserved for temporary relief where the system is widely affected.

The majority of cases where kidneys or bladder, or both, are involved, have unfortunately foci of disease elsewhere, and in many of them the early urinary symptoms do not lead to a definite diagnosis being always made, nor in many is direct operative interference beneficial; but in this field of surgery more particular examination by the cystoscope and the ureteral catheter promises in skilled hands to be of signal service, and I think we should frequently resort to inoculation tests in doubtful cases, and place less reliance on the often futile search for bacilli in the urine.

I do not hesitate to perform nephrectomy and ureterectomy even in advanced stages of disease where the other kidney is working, but have never been able to satisfy myself that partial removal of a kidney is of much value. So far as I am able to form an opinion, little if any benefit follows any direct attack on infected areas in the bladder, and I think that cystotomy for drainage should be reserved as a last palliative resort.

In all genito-urinary infections, considering the probable involvement of tracts far from the discovered lesion, and beyond the reach of any surgical measures, the advisability of a more extended and persistent use of tuberculin is well worthy of consideration, with a view to its thorough testing; though I confess that in the mixed infections so frequent in this region present experience does not warrant great expectations from its employment. Still, the well-known fact of possible recovery in both the orchitic and renal regions without local extirpation emphasizes the great importance of utilizing fully all methods proved to be anti-tubercular, and their continuance, even in the more advanced lesions, where palliative operations only are indicated.

Despite the fact that well-marked infections of the peritoneum may subside under general treatment, and the undoubted risk of parietal infection after section, the good results obtained by direct surgical operation in certain groups of cases are unquestionable. As was first demonstrated by Sir Spencer Wells, we still find recovery following the free evacuation of ascitic fluid by direct incision, and that the ablation of such organs as the tuberculous appendix or fallopian tube has a distinctly beneficial, if not curative, effect. On the other hand, where the exudate is chiefly of a plastic nature, with matting of the intestines by fibrous deposit, no good result follows abdominal section; in fact, harm is likely to follow. My experience of tubercular affections of the brain emphasizes the rarity of a single tumour possible of removal with advantage to the patient, and the comparative frequency with which wide dissemination precludes any possibility of useful local interference in both brain and cord tumours.

Primary tuberculosis of the skin comes so seldom under notice in South Australia that two typical instances may be quoted—one in which a boy accidentally wounded his hand whilst carrying a bottle containing tubercular sputum, a typical ulcer developed a month or two after in the scar; the axillary glands were involved; but, on making a clear sweep of all the infected areas, recovery ensued, and no further development took place after the lapse of three years. The other, in the person of a valued friend, whose hand was pricked whilst conducting an autopsy on a tuberculosis subject. A typical anatomical tubercle, which formed a few weeks later, was widely removed. Bacilli were found in the section, but no secondary effects have manifested themselves during the years following.

South Australian experience contradicts Stelwagon's assertion that true lupus is quite common in Australia. I rarely see it, and find that in the occasional cases treated by my colleagues who are engaged in radiography very encouraging results are attained by prolonged treatment with the X-rays. But this method, and that of Finsen, when obtainable, should be reserved for inoperable cases, and, in general, I am convinced no time should be wasted in the application of local caustics or the use of tuberculin, and that, in the face or elsewhere, free wide excision by the knife gives the best results. Supplemented by skin grafting, I would strongly advocate this practice in the early stages of the disease.

In this transient reference to surgical tuberculosis, its incidence and treatment, there has been adduced a little, if anything, novel or foreign to our common experience. I have not been able to indicate anything definite as regards a specific cure; nor do I anticipate that in the near future we shall attain any easy way, any direct approach, by which we may expect in entire confidence the winning of a complete victory over the protean forms of disease caused by the bacillus; but the retrospect already shows that in some regions of the body processes, formerly little altered by treatment, may now be more certainly modified, or even abolished; that the recognition of the actual cause has led to a whole series of methods most useful in diagnosis, to a world-wide activity in the laboratory and at the bedside, already giving much promise of being ultimately rewarded by the discovery of vaccines antitoxins, or other derivatives from the culture of the bacillus, which may, some of them, be of signal service by stopping the growth of the parasite in the tissues, and enabling the organism to eliminate its toxins.

There is now, as never before in the history of medicine, a lively expectation that the researches of the bacteriologist will furnish us with efficient agents in the treatment of both pure and mixed infections, and, encouraged in many directions by what has already been achieved in various regions of surgical work, we have some reason to expect even better results, not only in them, but in other parts and other conditions, which are now not infrequently beyond amelioration, for we recognise that—

"Man knows partly, but conceives beside,
Creeps ever on from fancies to the fact,
And in this striving, this converting air
Into a solid he may grasp and use,
Finds progress"

In the present session of this Congress we have already had the opportunity of hearing this branch of the subject treated by men whose work, largely inspired by Wright, is being closely watched throughout Australia; men to whom we look for very material aid in the future treatment of all forms of tuberculosis; and there is good indication of that aid

becoming of increasing value as their knowledge of the intricate biochemical relations between the bacillus and the human body becomes more and more clear.

If I have been able to emphasize the wide prevalence of tuberculous infections in a country which, so far as we know, was once immune, and to aid at all in stimulating endeavour directed against their extended continuance, I shall not perhaps have occupied your time and attention in vain, and, in any case, I thank you for your courteous and patient hearing.

A CONTRIBUTION ON THE TECHNIQUE OF OPERATIONS ON THE BILE PASSAGES AND THE PANCREAS.

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I shall not attempt, in the short time at my disposal, to do more than emphasise some important points, and direct attention to practical considerations which contribute materially to successful results. The works of Mikulicz, Moynihan, Mayo Robson, Kocher, and W. J. Mayo are the ones on this subject with which I am most familiar, and in my practice I have endeavoured to carry out their teaching. Certainly the modern methods have revolutionised this branch of surgery, and we can now with confidence and comparative ease perform operations which formerly were approached with misgivings, often seemed well-nigh impossible, and always caused infinite trouble and the loss of much precious time. I wonder what man among us can look back upon his operations in cases of cholelithiasis, say, ten or fifteen years ago, and feel confident that he was able, through the incision then in vogue, to thoroughly explore the ducts and detect the presence of a calculus, did one exist. On account of symptoms persisting I have had many opportunities of exploring the abdomen at various intervals of time subsequent to a cholecystostomy performed by myself and by others, and have removed calculi from the common bile duct, which I am satisfied were present but had been overlooked at the previous operation. One reason for failure in the original exploration may be attributed to numerous and dense adhesions, but, still more important, is insufficient access to the affected area owing to the incision, which most completely exposes this region, not being employed. All those who have followed the technique so strongly advocated and carried out with such brilliant results by Moynihan and Robson, cannot fail to be impressed by the immense assistance their suggestions offer. I have adopted their methods ever since their first work appeared, and many a time have felt devoutly thankful for their guidance.

The importance of Mayo Robson's modification of Bevan's incision, which I presume most surgeons now employ, and wonder how they got on without it, cannot be over-estimated. It simplifies in a remarkable manner procedures which otherwise would prove most complicated and tedious, often impossible. I will acknowledge that Kehr's bayonet incision, so much in vogue on the Continent, gives equal access. Robson's instructions are:—The upper end of the cut starts at the costal margin and extends vertically downward for 4 or 5 inches through the right rectus

muscle, close to its outer border. When it is necessary to open the common duct, or the deeper part of the cystic duct, prolong the incision upward, running parallel with the right costal margin, but about half an inch away from it in the direction of the ensiform cartilage, very freely exposing the upper surface of the liver. The gall bladder may then be grasped and firm traction exerted until the liver is drawn downward from under cover of the ribs, when it will be found that the lower border can be rotated and the cystic and common ducts brought quite close to the surface. Exceptions are very obese women. As Robson says, it will now be observed that instead of the gall bladder and cystic duct making a considerable angle with the common duct, an almost straight passage is found from the opening in the gall bladder to the entrance of the bile duct into the duodenum.

Wheelock Elliot, of Boston, first demonstrated the importance of a large sand-bag under the patient's back, about the level of the liver, which makes the operation area much more accessible and the intestines fall away towards the pelvis. The Lilienthal bridge attached to the operating table enables a similar advantage to be obtained, and after the patient is under the anæsthetic it can be elevated to the height required with ease and rapidity. The importance of filling the upper part of the right kidney pouch with a large flat gauze pad, which is packed in between the duct and the abdominal wall, cannot be too strongly insisted upon, as it then offers adequate protection against leakage from the open bladder or ducts. A second pad must lie to the inner side of the common duct above the stomach, and a third is placed to protect and cover the intestines.

If no adhesions are present the various steps of the operation necessary can be carried out with celerity and ease, but as so frequently happens they are dense and generalized, and, to use Moynihan's graphic expression, "bewildering in their infinite complexity." When the abdomen is opened in extreme instances, the surgeon may be excused if he feels that the tangle is hopeless, and impossible to deal with satisfactorily. However, it is wonderful what a degree of daylight penetrates after the judicious use of the scalpel, and how the field of operation is opened up, and anatomical relations can once more be recognised. I have met with many such cases, and so far have not failed to reach the duct and remove the impacted calculus or calculi without doing any deadly damage. The most difficult case I ever encountered, where the adhesions were so dense and numerous that I almost despaired of success, was operated upon by me in the Adelaide Hospital last December. The omentum had been washed back and was closely adherent to the anterior border of the liver and glued over a large portion of its anterior surface. Adhesions of a very dense and intricate character existed between colon, stomach, and duodenum. By the exercise of considerable care and patience these were overcome, the duct exposed, and calculi palpated and removed through an incision in the supra duodenal portion. Six were extracted, the largest being the size of a Spanish olive. Several were milked back from the retro-duodenal portion of the duct, enabling me to secure them through the high opening. In this case the importance of freely tilting the liver and straightening the ducts was well demonstrated, for the stones in the duodenal end of the duct seemed firmly fixed there, until Robson's manœuvre was completely carried out. Then I was able to gently work them back until the opening in the passage was reached.

It is strange, but nevertheless a fact, that this man had been explored by a surgeon in another State some months previously with negative results.

In reply to interrogatories, the following report was received:—"In September last, 1907, an exploratory laparotomy was performed, the gall bladder and ducts were free, but the substance of the liver was found cirrhotic. He improved after the operation and remained well, except for weakness, until he was discharged in October, the diagnosis being cirrhosis of the liver."

If gall stones are present in the upper part of the common bile duct it may be readily opened, after the duct has been brought near the surface by Robson's manœuvre, and the concretions extracted. The duct should be freely incised in its long axis, and its whole extent carefully explored, with the finger if possible, and with scoop and probe. Generally in old-standing cases the duct is so dilated that it will readily admit the index finger, which will promptly detect any remaining calculi or *débris*.

The question of drainage is a most important one, and it surprises me that there can be any diversity of opinion on this subject. I have never opened the duct or performed cholecystectomy without making provision for thorough drainage, and in a long series of cases my results have been most gratifying, and I am not likely to alter my technique in this respect. Mayo writes:—"If the cystic duct is widely dilated so that a rubber tube can be carried through the gall bladder and cystic duct into the common duct, the incision may be completely sutured—not otherwise. Free drainage of the bile to the surface contributes more to the safety of the patient than any other one feature of the operation. Drains are usually removed at the end of a week. Sometimes they are left in from ten to twelve days." I notice Robson always sutures the duct, and his method, of course, commands respect. Moynihan says:—"In my own early cases I not infrequently stitched the wound in the duct, but in a series of twenty-two consecutive cases I have drained the duct, and all the patients have recovered." Hartman considers that suture of the common duct is "always unnecessary and sometimes harmful." Quénu has abandoned suture entirely. Schwartz advises drainage in all cases, and many others are equally emphatic. With the modern method of operating, sutures of the duct can be done easily and quickly, but I cannot agree with this step, which certainly prolongs the operation to some degree, and is attended by no compensating advantage that my experience leads me to recognise. It is rare for stricture of the duct to follow the operation, but I can imagine it is more likely to occur after suturing than when the opening in the duct has first to carry a drainage tube and gradually contract subsequently. Of course, the main object in free drainage, as first pointed out by Kehr, is to get rid of all septic organisms as thoroughly as possible from the liver.

At this stage I may briefly refer to three cases which, I think, are of interest, and which I select from a series operated upon by me this year in the Adelaide Hospital.

Mrs. C., aet. about 50, was operated upon on the 16th September, 1908, having had characteristic symptoms of chole-lithiasis and gallstone colic extending over two years. She was a heavy woman with a fat abdominal wall, but organically healthy. The usual incision and rotation of the liver enabled me to explore the gall bladder and ducts thoroughly. Adhesions were not numerous, nor was there any marked alteration in the walls of the gall bladder. Numerous calculi could be easily palpated. After shutting off the affected area with gauze pads I emptied the gall bladder of its fluid contents with a hollow needle, and then through a free incision removed sixteen faceted calculi, about the size of small

cherries. One in the cystic duct, close to its junction with the hepatic and entangled in the valves of Heister, could not be removed at this stage. A rubber drainage tube was inserted and fixed in the opening by a double layer of purse-string sutures in the usual manner. I then opened the common duct in its supra duodenal portion, and could easily introduce my forefinger for exploratory purposes. Six calculi extending as high as the primary division were removed from the hepatic duct, and the same number, all about the size of those in the gall bladder, were extracted from the retro-duodenal portion of the common duct, which was considerably dilated. Finally, the stone impacted in the cystic duct was delivered, and after arrangements were made for free drainage the wound in the abdominal wall was sutured in the greater part of its extent. At the end of a week the drainage tubes were removed and the patient recovered without a bad symptom.

On the 16th July, 1908, in the Adelaide Hospital, I operated upon an old woman, aet. 65, with symptoms of gall stone colic extending back six years. She was distinctly jaundiced. When the abdomen was opened many adhesions were found between the duodenum and transverse colon, and what at first appeared to be the gall bladder. Actually the gall bladder was shrivelled and almost obliterated, and the sac at first mistaken for it. It proved to be an immense dilation of the common duct. It contained two gall stones, which were removed. One was the size of a large walnut. The other was smaller and flattened out. The usual measures to establish free drainage were taken, and the patient made a rapid and uninterrupted recovery. In this case I might have performed choledocho-duodenostomy with advantage to make certain of permanent and free drainage, but the method employed was apparently attended by all the success desired.

E. S., aet. 56, married woman, was operated upon for gall stones five years ago, and obtained relief from all her symptoms for four and a half years. At the termination of that period she had a sudden attack of acute pain in the R. hypochondrium with vomiting and subsequent jaundice, clay-coloured stools, and biluria. Similar attacks occurred at intervals of several weeks, and on the 20th April, 1908, she was admitted to my ward. On the 28th I operated, and, as might be expected, found adhesions between the gall bladder and surrounding structures numerous and dense. These were with some difficulty separated, the common duct opened in the usual situation, and fourteen stones and some *débris* were removed. When the duct was incised very free hæmorrhage occurred from the transverse vein which Mayo describes, but which I had never before encountered. I had no special difficulty about arresting the bleeding. The duct was not sutured, free drainage was established, and the greater part of the abdominal incision closed in layers as usual. On the 29th tubes were removed, and on the 27th May the wound was firmly healed and the patient discharged.

Not long since, when operating upon a gall stone case, a medical man present stated his belief that in the vast majority of cases of cholelithiasis surgical interference was unnecessary. This remark came to me as a rude shock, for I was under the impression that every modern practitioner was convinced by his own practical experience, and by the recent literature on the subject, and would be a strong advocate of the "No delay" policy. My friend seemed to think that because I had been for many years a surgeon to a large hospital I had developed the itch for operating, hence was prejudiced. I replied that I considered it just as important to

operate on a case when the presence of gall stones was diagnosed as to remove promptly an offending appendix, and very few men who keep themselves abreast with the times will think of arguing in favour of medical treatment when the classical signs of a diseased appendix present themselves. When we realise the complications and disasters that follow a masterly inactivity, we feel that too much stress cannot be laid upon the rule that prompt action is urgently needed, and to advise operation is the medical attendant's imperative duty. In these days of aseptic surgery and improved technique the results are almost uniformly admirable, even in complicated cases, and that, with the many examples constantly presenting themselves of the dire results from delay, must immensely increase our confidence in recommending the use of the knife. Now that we understand more about the pancreas, and realize how very frequently pancreatic disturbances, both of an acute and chronic character, are occasioned by the presence of gall stones in the common duct, the veriest sceptic must be convinced that surgical treatment is the safest and the surest method of obtaining permanent relief.

W. J. Mayo lays special stress upon the careful arrangement of viscera before the closure of the abdominal wound, with the object of keeping the small intestines from coming in contact with the infected area. The protection is furnished by the upper edge of the omentum and the transverse colon. These structures are carried upward and placed between the gall bladder and ducts and the abdominal cavity, and if necessary they can be held in position by one or two catgut sutures.

In certain cases where the adhesions are numerous, complicated, and exceptionally dense, burying the shrunken gall bladder, and perhaps cutting off its communication with the common duct, time may be saved and a good result obtained by carrying out the manoeuvre suggested by Moynihan, which he terms rotation of the duct. The stone in the duct is localised and the surgeon's left hand passed transversely inwards in front of the pylorus, and above the stomach along the gastro-hepatic omentum. The fingers are flexed and the duct twisted up into the wound, the stone thus rendered prominent and an incision can be carried down to it directly through the adhesions. Of course the area is well shut off by carefully arranged swabs to catch the bile which usually flows very freely, and the process facilitated by two parallel sutures firmly planted in the long axis, which are useful in helping to fix the duct, and afterwards embrace the drainage tube and keep it in position. I have never tried this method, but have preferred to follow Mayo Robson, who writes—"If adhesions have been thoroughly separated, as they always should be, the surgeon has immediately under his eye the whole length of the duct with the head of the pancreas and duodenum." In suitable cases for the plan, so highly eulogised by Moynihan, the difficulty is to be perfectly satisfied that adhesions only are cut through and no important structure unwittingly divided.

It is very seldom that retro-duodenal choledochotomy is required, as a calculus here situated can generally be worked up into the supra-duodenal portion where the duct is usually opened. When it is necessary, the operation is facilitated by Kocher's suggestion of "mobilising the duodenum," which, according to Mayo, has a very small field of usefulness. I must say that I found this method very valuable in two cases when the stone was impacted firmly in the second portion of the duct, and could not, by any means known to me, be coaxed or forced into the more accessible part of the canal. I was well satisfied by this method which Moynihan describes

as follows:—"The parietal peritoneum of the posterior abdominal wall is vertically incised about $1\frac{1}{2}$ inches to the right of the duodenum. The fingers are introduced into this incision and the peritoneum is stripped up until the duodenum is reached, by dragging gently on the second part of the duodenum it can be turned over to the left so that its posterior surface is visible. A stone seated in the second portion of the duct can now be felt and the duct over it incised. The pancreas surrounding the stone is probably atrophied or sclerosed by reason of the constant pressure of the gradually enlarging stone and is therefore only recognisable as thick tough fibrous tissue."

Duodeno-choledochotomy is required if the stone is impacted in the ampulla and cannot be brought up to the seat of election. As far as I can gather the consensus of opinion favours this operation rather than opening the retro-duodenal part of the duct. The stone if it cannot be milked into the upper part of the duct can often be squeezed into the ampulla when it can be extracted through the duodenum as follows:—The stone in the ampulla is grasped with the thumb and forefinger of the left hand and brought close up against the anterior wall of the duodenum, which is now incised, and the escaping secretions carefully mopped up. The margins of the cut in the duodenum are grasped with forceps on each side. An assistant gently pulls these apart, and the stone can be plainly seen lying behind the tense mucous membrane of the posterior wall of the duodenum—just above the papilla. The overlying mucous membrane is incised, and the stone extracted. As a rule it is not necessary to put a stitch in the posterior incision unless it has been extensive, in which case it should be of catgut tied on the mucous surface. The anterior duodenal wall is then closed with two rows of sutures and the abdomen may be closed without drainage.

I have never performed this operation as I have hitherto been fortunate enough to succeed in coaxing the stone back into the supra-duodenal portion of the duct, except on two occasions when I performed retro-duodenal choledochotomy. It is evidently very seldom necessary, as I notice that the Mayos of Rochester have had to adopt the transduodenal method for the removal of a calculus in four instances only in over 2,000 operations upon the gall bladder and ducts.

After studying many specimens and discussing the subject with Professor Watson, I rather incline to his view—That when calculi are found in the retroduodenal portion of the duct, which, under such conditions, is almost invariably dilated, they should be removed by the duodenal route. This is what Kocher suggested and first carried out in 1894, and was described by him as internal choledocho-duodenostomy. Moynihan very fully describes it in his work. Squeeze them forward rather than back—open the duodenum incise the papilli of Vater upward, and release the stones. This method is not attended by any additional risk and has the advantage of establishing free drainage of bile into the bowel and eliminating that fibrous collar, almost amounting to a stricture, so frequently present at the duodenal extremity of the duct. The danger of dividing the superior pancreatic duodenal artery must not be overlooked should this operation be carried out. I cannot quote any personal experience of this as I have never given it a trial, but believe that it will be very much more generally employed in the future than it has been in the past.

Watson is a firm believer in the advantage gained by incision in the right loin, the utility of which in many cases is very evident. It may prove of material assistance in clearing away any doubt associated with the

diagnosis, the left forefinger introduced through the opening is often a decided help in milking the stone back from the retro-duodenal portion of the duct, and this cut is always valuable as an aid to efficient drainage. Watson is such an enthusiastic supporter of this procedure that he recommends its adoption as a routine practice, suggesting that it should be employed as a preliminary measure before the anterior wall is incised. I have often satisfied myself of its value, but in a case recently operated on by me it failed as a help to diagnosis. The history was indefinite, but I suspected cholelithiasis, and exploration was agreed to. I could not reach the gall bladder from behind on account of a very pronounced Riedel's lobe, the gall bladder being pushed out of its usual position, and, in spite of the falciform ligament, was lying to the left of the middle line. When exposed through an anterior incision it was found to be packed with stones, but the ducts were free.

The lymphatic glands lying contiguous to the common duct are not numerous, but they may, owing to inflammatory changes, cause pressure and give rise to symptoms. Two glands are constant. One at the junction of the cystic and hepatic ducts, the other in close opposition to the outer wall of the lower part of the supra-duodenal portion. I have operated upon two cases of long standing icterus, in the belief that I had to deal with duct obstruction due to gall stone, and when the affected area had been exposed I found the duct was pressed on from without owing to enlargement of a lymphatic gland. Both cases did well and recovered completely, but I could not get the glands away without removing a portion of the duct wall, as they were so firmly adherent.

The last case coming under this category was that of a very jaundiced old man, aged 65, whom I operated upon in the Adelaide Hospital in October, 1906. The adhesions were general and numerous around the duct, and it was some time before I could remove an irregular chalky mass, surrounded by a dense fibrous capsule, which was glued to the duct wall, and which was undoubtedly a gland undergoing calcareous degeneration. He made an uninterrupted and satisfactory recovery, and all symptoms disappeared.

Acute pancreatitis, in the majority of cases, is caused by gall stones directly or indirectly, and should be dealt with surgically at the earliest possible moment. If the gland is tense and acutely inflamed it should be treated on the lines of other phlegmonous inflammations and freely incised. It was first deliberately practised by Muspratt, in 1903, a complete recovery following in a case where the pancreas was purple and extremely tense, with numerous spots of fat necrosis existing. Robson has collected 59 cases of operation during the acute stage with 23 recoveries, a large mortality, certainly; but how many cases would have died had expectant treatment alone been carried out? Free incision with suitable drainage is evidently the best practice, and this should be carried out at the earliest possible moment, before toxic influences are pronounced; the operation being completed as speedily as possible, to reduce shock and complications through the anæsthetic to the minimum.

Robson has had six cases of acute pancreatitis under his care, and operated upon four, of which two recovered.

I have only seen two cases operated upon. One by a colleague in the Adelaide hospital and one I had in private. They both came under the surgeon's notice too late, and a fatal result was recorded in each instance.

The collections of pus which are usually associated with the subacute variety of this disease may be localized, and bulge in the upper part of the abdomen, having relations with the stomach and transverse colon, similar

to those so well known in connexion with pancreatic cysts. The pus may escape from its lymph barriers and the anatomical relation readily explains the course along which it burrows. Robson advises that after the pancreas has been explored by means of a median incision placed above the umbilicus, it may be practicable to evacuate any collection present by a posterior incision in the left or right costo-vertebral angle. If this is impossible then, after the cavity is emptied of its purulent contents by aspiration, it may be opened and packed with gauze, which should be brought forward through a large rubber tube, and thus in from one to two days a track isolated from the peritoneal cavity is established. Robson has collected fourteen cases of abscess resulting from subacute pancreatitis, which were subjected to operation, with five deaths, giving a mortality of 35.6 per cent.

If gall stones or pancreatic calculi have given rise to chronic intestinal pancreatitis, their removal will usually suffice to clear the ducts and admit free flow of the secretions. Should, however, the trouble originate some other way the changes in the pancreas may be arrested or cleared up entirely by the establishment of permanent free drainage, best effected by cholecystenterostomy whenever it is possible to carry out that operation. This measure is not of any value in obstruction caused by malignant disease, but when from some cause that cannot be removed the free passage of fluid contents in the duodenum is interfered with, cholecystenterostomy is the operation *par excellence*.

Robson and Moynihan both prefer to make the anastomosis by suture in exactly the same way as a posterior gastro-jejunostomy is performed. The communication may be easily effected with a small Murphy's button, which yields very good results. This operation in Robson's hands has been attended by brilliant results with a mortality a little over 2 per cent. Now that the beneficial results of surgical treatment have been so clearly demonstrated and the value of this procedure conclusively proved, it behoves surgeons to exercise increasing watchfulness and employ every effort to get these cases in a fairly early stage, when a satisfactory result may be confidently expected. Later, when the interstitial processes are more advanced, when the islands of Langerhans have been implicated, bringing about glycosuria and serious constitutional changes, only some degree of amelioration can be hoped for when this operation is performed. In that very comprehensive work on the pancreas by Robson and Cammidge several cases bearing on this point are given *in extenso*.

On the 13th August, in the Adelaide hospital, I operated upon a woman, aged 36, who had been jaundiced for two years, but there had been no attacks of gall stone colic, no progressive emaciation, no distended gall bladder, no cachexia. The gall bladder was healthy, the ducts easily palpated and free from any concretion, the head of the pancreas was markedly enlarged, hard, but not nodular, and evidently causing pressure upon the common duct. The glands along the lesser curvature of the stomach were enlarged, but discrete, not confluent, and two enlarged lymphatic glands were found, one at the junction of the common and cystic ducts, and the other at a lower level. I decided that this was almost certainly a case of chronic pancreatitis and that constant drainage obtained by a cholecystenterostomy was indicated. I made the juncture between the gall bladder and the second part of the duodenum, employing sutures, and closed the abdominal wound without drainage. The patient made an uneventful recovery, the jaundice gradually but surely cleared away, she put on flesh rapidly, all symptoms of disordered digestion subsided, and a satisfactory return to usual health may be recorded.

Dr. Angas Johnson has kindly interested himself in Cammidge's "A" and "B" pancreatic reactions at my suggestion, and has carried them out with great care and accuracy. The results so far have been successful, but we shall require further experience and acquaintance with the tests before being able to bear independent testimony to their diagnostic value. Robson and Cammidge have apparently proved that most important information can be gained in case of pancreatic disease by the methods of examining the urine they so strongly advocate.

I have given chloride of calcium an extended trial in doses of 40 grains per diem before and after operating, with the view of increasing the coagulability of the blood, and diminishing hæmorrhage in cases where jaundice had been a prominent and persistent symptom, but cannot say I have been impressed by its advantages. Mayo Robson still strongly advocates its use before and after operation, but Moynihan and others have satisfied themselves that it is of no value, and my experience of its use prompts me to agree with their opinion.

Crowe, of Baltimore, has satisfactorily demonstrated that urotropine administered by the mouth exercises a marked bactericidal action upon the bile without giving rise to any gastro-intestinal disturbance, even when 80 grains a day are administered. I have often given 40 grains per diem for a lengthened period, as a urinary antiseptic without observing any unpleasant symptoms, and if all micro-organisms can be removed from the gall bladder in this manner, the surgeon would be wise to advocate its employment in operations upon the bile passages. It may be a valuable suggestion, and I notice that Dr. Fiaschi, of Sydney, writes—"My own experience, gathered from various cases of gall stone operations, is that the administration of urotropine, even in doses of 15 grains a day, is distinctly beneficial." Before operation, and during the after treatment, the use of urotropine is recommended with the object of rendering the bile sterile. I hope later to have some practical experience in this respect, but at present can claim no personal knowledge of its advantages.

After Treatment.—Warm gastric lavage is an excellent measure and the removal of accumulated fluids from the stomach affords the patient immense relief.

I have for some time past made it a rule to carry out Murphy's suggestion and use the drip method of introducing saline solution into the rectum, and am satisfied that decided benefit follows its use. In all abdominal operations accompanied by considerable shock, recourse to this practice has much to recommend it. Mayo remarks that he has found Murphy's method to be of great value when patients are in a bad condition from jaundice, &c. He introduces into the system in this way about 6 quarts of water in the first 24 hours, and notes that the kidneys begin to act freely and the pulse rapidly improves.

I am aware that certain distinguished Australian surgeons are opposed to proctoclysis, even in cases of perforative peritonitis, maintaining that its adoption is of no material advantage, and the patient is worried and inconvenienced by the apparatus. As my experience has led me to express myself very strongly in its favour, I take this opportunity of quoting Murphy's directions for its successful employment.

"Moderate distension is the normal condition of the large intestine. If it is hyperdistended it causes spasm and expulsion of material. The mucosa of the large intestine absorb water with great rapidity. We have administered 30 pints of normal salt solution in 24 hours to a patient eleven years old, and it was all retained. The retention of fluid in the colon

depends entirely upon the method of administration. We have visited hospitals numbers of times and have been shown patients who were receiving 'the Murphy treatment.' We should not have recognised it without the label. It is difficult to impress nurses and doctors with the importance of details, notwithstanding that the best results are secured by close attention to detail. The fluid should be administered through a fountain syringe, to which is attached a $\frac{3}{8}$ -inch rubber hose, fitted with a hard rubber or glass vaginal douche tip, with multiple openings. This tube should be flexed almost to right angles, 3 inches from its tip. A straight tube must not be used as the tip produces pressure on the posterior wall of the rectum when the patient is in the Fowler position. The tube is inserted into the rectum to the flexion angle and secured in place by adhesive strips binding it to the side of the thigh so that it cannot come out; the rubber tubing is passed under the bedding to the head or foot of the bed to which the fountain is attached. It should be suspended from 6 to 14 inches above the level of the buttocks and raised or lowered to just overbalance hydrostatically the intra abdominal pressure. The flow must be controlled by gravity alone and never by a forceps or constriction of the tube so that when the patient endeavours to void flatulence or strain, the fluid can rapidly flow back into the reservoir, otherwise it will be discharged into the bed. It is this ease of flow to and from the bowel that insures against over-distension and expulsion on to the linen. The fountain had better be a glass or graded can, so that the flow may be estimated. The temperature of the water in the fountain can be maintained at 100 deg. F. by encasement in hot water bags. The fountain is refilled every two hours with $\frac{1}{2}$ to 2 pints of solution. The tube should not be removed from the rectum for two or three days. When the nurse complains that the solution is not being retained, it is certain that it is not being properly given; even children can tolerate proctoclysis surprisingly well. It rapidly restores blood pressure, it improves the capillary circulation, it quiets the thirst, it eliminates the septic products and increases the excretions. All the details are simple, but they must be carried out with precision to secure the best results."

I feel that I have only been able to lightly touch the fringe of this vast subject. So many important matters have not even been alluded to, such as malignant complications, pancreatic cysts, pancreatic calculi, &c., and, so far as this paper is concerned, they must be neglected; but I trust others will contribute interesting experiences on these points before our Congress closes.

THE SURGERY OF THE BILE PASSAGES AND PANCREAS.

DIAGNOSIS AND INDICATIONS FOR OPERATIVE TREATMENT.

H. CRITCHLEY HINDER, M.B.

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I have operated upon 180 cases of disease of the gall bladder and biliary passages, so that rather than follow in any beaten track I thought that it would be best, in the limited time at my disposal, to place before you the opinions I have formed from observation of my own patients, rather than review the work of other men, though they may be far more competent than myself to speak authoritatively on a subject of such vast interest to both physicians and surgeons.

In the first place, I firmly believe that we must divest ourselves of the idea that biliary colic is due to an effort on the part of the gall bladder to force a stone along the biliary passages, and that the cessation of pain is an indication that it has effected its object. We know that, as a rule, the stone is not expelled when the pain has ceased, and that the stone is often of such a size that it remains for a considerable period within the gall bladder or the bile duct, and that therefore it is more probable, in fact fairly certain, that the pain is due to a temporary blockage and the consequent distension of the gall bladder by fluids which are unable to escape.

Occasionally adhesive peritonitis is set up between the gall bladder and surrounding structures, so that a certain amount of pain must at times be attributed to this source; but its character will hardly be that of a true biliary colic.

Before one can speak of the surgery of the bile passages, we must make fairly definite, our conception of the general principles underlying the pathological changes, which give rise to the common clinical symptoms known to us as liver attacks, biliary colic, or whatever other terms we may apply in order to satisfy our own misunderstanding, or to supply an explanation to the urgent desire for knowledge on the part of our patients.

I believe that the first step in the pathological chain is, that a something brings about an obstruction to the escape of bile and gall bladder secretion, whether this is due to an infection of these passages in every case, or whether the secretions themselves become altered so as to irritate the lining membrane of the passages, and thus produce swelling and obstruction, remains yet to be proved. It is certainly hard to believe that these infective processes should implant themselves upon a normal membrane, unless the soil had in some way been prepared for their reception. Every such attack brings about the production of an excess of dead epithelium, a biliary stasis, and in fact all those conditions which go to favour the formation of calculi. In opening a large number of gall bladders, we find that there is laid before us every stage in the formation of stone; thickened viscid bile, tiny granules, bile mud, and stones of varying degrees of consistency.

In bringing our thoughts to bear on the diagnosis of disease of the biliary passages, we must remember that we are dealing with an infective process, and that the pain is almost certainly due to the common cause of most pain, namely, tension, and that this tension is brought about by the inability of the free normal secretion to escape as rapidly as it is wont to escape, and also that jaundice, though a valuable diagnostic sign when present, is merely an accidental concomitant, and its absence is of no significance whatever.

Acting upon this assumption, we will have no difficulty in explaining the symptoms of what is commonly spoken of as biliousness. The dull heaviness in the hepatic region, the throbbing head ache, the feeling of nausea, the sallowness with almost an icteric tinge, are all due to toxic absorption, partly cholæmic, and partly due to the toxins of organisms usually found in infection of the biliary passages. During such attacks, the pain experienced is not very great, because the amount of obstruction is not great, but with a frequent repetition the obstruction is apt to become more and more complete, and as a greater amount of distension occurs, a greater pressure is necessary in order to re-establish a free passage, so that in course of time, an attack of some severity, a true biliary colic

is brought about. It very often happens that patients become so accustomed to these attacks of so-called biliousness, or acute indigestion as they call them, that the medical man does not see them until an attack of great severity is experienced. Blame is frequently attached to various articles of diet, but on inquiry it will be found these very same articles have been eaten at other times with impunity. During these mild attacks a moderate amount of tenderness is always experienced over the region of the gall bladder and in its immediate vicinity. This tenderness, bear in mind, will certainly depend on the build of the patient and the ability of the examining hand to reach the seat of trouble. It is hardly to be expected that the gall bladder will be felt in these early stages of the disease, for unless it be considerably enlarged and very tense, it will certainly escape notice unless in a very favorable subject. In the later stages of the disease the symptoms are certainly more marked, and although I have seen a patient with a few very large gall stones, who stated that she had only suffered from occasional attacks of pain, yet the attack for which she sought advice disclosed, on operation, a suppurating gall bladder which had ruptured, and yet she had only sent for her family attendant the night before, so that in many of these anomalous rapidly progressing cases, we must not fail to take into consideration the personal equation of the patient. I have operated upon a few patients who gave a history of attacks of agonising pain, and yet no gall stones were found, but merely an enlarged gall bladder slightly thickened, and these same patients have since passed several years in comfort, and with complete absence of pain.

Both these pathological extremes merely force us to the conclusion that the degree of the intensity of the attack, as evidenced by the patient's behaviour, is certainly not to be wholly depended upon as a guide of any great value.

I have been at pains to discover if it would not be possible to diagnose the situation of the obstruction by the character and situation of the pain; but although an obstruction in the common duct usually gives rise to pain which is described as being central and boring through to the back, I have also found a distended gall bladder without any obvious obstruction in the common duct, give rise to precisely the same kind of pain. All we can say with certainty then, is, that the pain which accompanies disease of the gall bladder or bile ducts is usually situated in the neighbourhood of the gall bladder, and that it extends radially for a few inches, and that it is very frequently described as going through to the back.

Jaundice, the presence of bile in the urine, and its absence in the fæces, are undoubtedly of assistance in the diagnosis, but the absence of these signs is of no value whatever, because a very large percentage of patients suffering from disease of the biliary passages never suffer from jaundice.

Vomiting is a symptom which may be present in any abdominal condition, and is merely indicative of the fact that great pain is being felt by the patient, or that there is a rapid absorption of toxic products.

I shall now proceed to speak of cases which have occurred in my own experience, and which go to show how easily we may be deceived in attempting to make an accurate diagnosis.

A diaphragmatic pneumonia or pleurisy may be extraordinarily like an acute cholecystitis, and I have heard of an operator opening the abdomen under the impression that he was dealing with a case of cholecystitis. The increase in the respirations is the most constant sign of warning, for it is possible to have a localized peritonitis and perihepatitis which will make

the case extraordinarily like one of diaphragmatic pneumonia, too deep-seated to show the ordinary clinical signs. I have repeatedly demonstrated the fact that the fundus of the gall bladder is often very close to the pelvis of the kidney in some patients, being very often only an inch away, and on one occasion a patient of mine suffered from colic which I thought was renal in origin, particularly as the kidney felt irregularly enlarged. I cut down through the back and actually opened into the gall bladder through the kidney before I was aware of it. Evidently the patient had suffered from cholecystitis, and the large gall bladder had fallen back, and had become firmly adherent to the lower end of the kidney.

The resemblance between a duodenal ulcer and an inflamed gall bladder is apt to be very puzzling. A woman of 30 years was under my care, and sometimes her pains appeared to bear some resemblance to duodenal ulcer, and at others to over distension of the gall bladder. On opening the abdomen, there was evidence in the shape of an old duodenal scar, that an ulcer had at one time been present, and that the gall bladder was adherent to this spot by a broad band of adhesions. This band was severed and the gall bladder shortened and attached to the abdominal wall. Since then she has had no further pain.

As the result of repeated inflammatory attacks, the gradual closing up of the neck of the appendix and the formation of calculi, will give rise to symptoms and signs which are very like those of a cholecystitis, and in five cases I have found both conditions in the same patient. When, therefore, we find an appendix high up in the abdomen, I think it practically impossible to say precisely why the patient is ill. I operated upon two cases of appendicular abscess in which the appendix was lying perforated right along the outer side of the gall bladder, so that the gall bladder helped to form the wall of the abscess, and yet it was apparently unaffected.

A hydatid cyst will sometimes produce symptoms of duct obstruction, and if the hydatid is small, its diagnosis is impossible. I have often found gall-stone disease associated with hydatid of the liver. Possibly some form of mechanical obstruction was produced. Twice I have removed gall stones from the gall bladder, unaware that any hydatid was present, and small daughter cysts have been extruded through the tube which was draining the gall bladder.

In the differential diagnosis of malignant disease, the outstanding features are the slow onset, the absence of pain and temperature, and the gradually increasing jaundice. Jaundice which becomes deeper and deeper, and does not tend to improve in any way, should generally suggest malignant disease. The absence of pain and temperature is not infallible, for I have known patients experience both; but they are certainly most unusual symptoms. A prolonged and intense jaundice is possible when a large stone fits tightly in the common bile duct, but such a happening would be unusual.

There are times when a patient may be seen with such abdominal distension vomiting and obstipation that it looks very like as if he were suffering from intestinal obstruction, but the obstruction is after all only a reflex. In these cases, it is as well to remember that cholecystitis is almost always associated with some rise of temperature, whereas intestinal obstruction which is not due to an acute inflammatory condition usually has none.

Tenderness over the gall bladder is always present in acute cholecystitis. In fact, the tenderness on pressure over the region of the gall bladder is

the most constant, the most reliable diagnostic factor we possess. We may sometimes make a rough guess that the patient has a stone in the common bile duct, but it is, to my mind, useless to attempt such refinements, for they are merely guess-work; but we certainly can take care to examine all cases of what are known as acute and chronic dyspepsia or biliousness, and make up our minds as to whether we are not dealing with a case of cholecystitis.

INDICATIONS FOR OPERATION.

If we have a patient who suffers from the early premonitory symptoms of cholecystitis when there is only a dull heaviness, with slight tenderness and discomfort, it might certainly be possible, by depleting him with salines, by the administration of antiseptics, and particularly by attention to his diet, gradually to restore to health his biliary passages which are becoming more and more altered by repeated inflammatory attacks.

If the disease has advanced so far as to give rise to an acute cholecystitis, then I certainly think that the greatest amount of good will be obtained by immediate operation. There is absolutely nothing to be gained by waiting, and much may be lost. I have seen four patients with a ruptured gall bladder and abscess. One of them was an old lady of 70 years, who had not suffered from a definite attack of biliary colic for years. Three other patients died of an acute septic cholangitis whose gall bladders already contained a few gall stones, and were constantly septic. These patients had been treated after the expectant method. When I was in general practice a few years ago, I lost five patients in one year from malignant disease of the gall bladder. All of these gave a history of occasional attacks of cholecystitis. The gall bladder should never be removed unless it is obvious that it has been so altered as to be unable to discharge its normal functions. I have only removed it seven times, and the last patient, a man of 70, has had a discharge of bile from the wound for two years, and, although I operated again in order to discover the cause, I was forced to the conclusion that he had some stricture of the common duct. Had he a gall bladder, an anastomosis might very easily have put him to rights. Such a possible contingency should always be borne in mind, and doubtless this latest fashion which obtains among some men of removing the gall bladder on all possible occasions will soon die out.

I have very frequently met with an enlarged and very dense pancreas when operating for cholecystitis, particularly in old-standing cases, where gall stones have been found. In fact, it has been very difficult at times to say whether it was a chronic inflammatory condition or malignant disease. The symptoms have never appeared to me to indicate anything unusual. I have only been able to discover the presence of sugar in four cases. The sugar disappeared from the urine of three of these in from six to fourteen days, but one patient improved very little.

I have always made a point of draining the gall bladder of patients in whom I have found chronic pancreatitis, for about two or three weeks, considering that this would be likely to have a beneficial influence over the recovery of the gland.

I met with one case of rupture of the pancreas. He was a patient of Dr. Grigson, of Muswellbrook, who told me that the man fell off the top of a loaded dray, striking the shaft. He was very collapsed, and vomited considerably for a few days. When I first saw him, about the

fifth day of his illness, he looked very ill, and had a temperature of 101 degrees, and a pulse of 140. There was a considerable swelling in the upper and left side of his abdomen. I discovered that this consisted of about 3 pints of bloody fluid in the lesser sac of the peritoneum. The digestive action of this fluid on fibrin made it fairly certain that it was pancreatic in origin. A large drain was inserted, and he recovered.

I certainly do not think that I have missed any cases of acute pancreatitis. A search has been made through the records of the Royal Prince Alfred Hospital covering the past three years, and only two cases of acute pancreatitis were admitted. They both died, and were in such a moribund condition that their history was not of much value. It was simply that of an acute abdominal attack. The patients died within a short time after admission.

From a study of several cases of acute pancreatitis, Dr. Bornhaupt, writing in the *Deutsche Medizinische Wochenschrift* for 23rd July, 1908, concludes that more frequent diagnosis ought to be made if certain characteristic signs of the disease be kept in mind in the presence of any acute abdominal symptoms. Pancreatitis of this type is a non-infectious condition, and therefore there is usually no fever accompanying it; indeed, the temperature is frequently subnormal. The absence of peristaltic movements, and of any marked meteorism, speaks against acute intestinal obstruction, which simulates most often the picture of acute pancreatitis. Palpation usually gives no distinct signs, and so puts one on guard against suspecting any condition with a well-expressed anatomical basis. The pain of pancreatitis is usually epigastric in location, is of extreme intensity, and is further characterized by its persistence in undiminished violence for longer periods of time than is usual in peritoneal inflammations. The presence of sugar in the urine is a sign of great corroborative significance, while the general collapse of the patient, from the unbearable suffering, is so extreme, that operation is usually consented to very quickly, and should in no case be long deferred in presence of suggestive symptoms. Prognosis in such cases is favorable only if the gland is partially diseased, and not enough time has elapsed to allow the absorption of a deadly dose of the toxic products of the gland. The pathology of the affection gives good indication for the procedures to be undertaken in operating. First of all, the toxic secretions must be allowed to escape without further destroying the surrounding tissues; secondly, the infiltration about the gland and consequent pressure upon the celiac plexus must be relieved by properly instituted drainage. With early diagnosis and, therefore, early operative treatment of the affection, the chances of the patient to overcome the effects of the acute hæmorrhage or necrosis of the pancreas are greatly bettered.

A CASE OF ACUTE RUPTURE OF THE GALL BLADDER.

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By acute rupture is meant the sudden giving way of the gall bladder from backward pressure of the bile. The process is essentially a sudden one, and the gall bladder tears at the part least able to bear distension, namely, at or very near its fundus. It is by no means necessary that the

ruptured organ should have contained stones; indeed, I believe their presence would tend to reduce the risk of rupture, but, if present, they are distributed among the structures which form the gall bladder and bile duct trough. The tear is a triangular one resembling that resulting from the forcible bursting of an inflated tissue-paper bag. The ruptured gall bladder is, as a rule, a small somewhat cicatricial organ, with few or no adhesions save to the surface of the liver above. It has necessarily lost its elasticity and capacity for distension which the normal gall bladder possesses. The accident would appear to be a very rare occurrence indeed. Mr. Bland Sutton* makes no mention of it. Mr. Moynihan† says he has only had two cases in his own practice, and he remarks, in recording fourteen cases, that, "The subject is so important, and so little is known of it, that he cannot do better than simply to record the reported cases." Mr. Battle‡ mentions one case of ruptured bile ducts; but this was from external violence, which is an entirely different matter, but he remarks on its rarity. I have myself operated on the case I am about to give in detail, and have seen one other in the dissecting-room, which Professor Watson kindly showed me. Apparently the last case passed unrecognised, as the patient died of the condition, and the whole of the right side of the abdomen was distended with bile, which had also made its way into the pelvis, and up the left side of the abdomen as high as the diaphragm—evidently the pressure of bile had been very great. Stones had been present in the gall bladder at the time of rupture, and they were picked out here and there along the course which the bile stream had taken. The gall bladder trough, formed by the liver above, the transverse colon below, the right kidney behind, the abdominal wall in front, and the duodenum and stomach on the inside, had been at first greatly distended, and the bile had only gradually broken through its adhesions and travelled downward and then to the left.

The essential condition which makes rupture likely or possible is some obstruction in the common bile duct, and this obstruction is, of course, in most instances, a stone which occasionally causes a complete blockage of the duct. The presence of this stone, although a predisposing cause, and an essential feature of rupture, is not the factor which directly brings it about; something else is necessary as a direct exciting cause. In my own case, I think what brought about the accident was the fact that the patient had a violent cough; he had, too, partaken of an unusually large meal, and he had a loaded transverse colon.

The spasmodic contractions of his diaphragm and of his abdominal muscles had so squeezed his cicatricial and distended gall bladder between his liver—to which it was densely adherent—and his colon, that that organ, unable to empty itself through the duct of Vater, had given way at its least protected position, viz., the fundus. Some explanation is necessary to account for the enormous pressure of bile found within the abdomen; but the reason for this is obvious. The pressure is not that simply of the secreted bile, but of the amount of pressure which can be exerted by the expulsive action of the contracting diaphragm and abdominal muscles in the act of coughing. This constantly squeezed out more bile through the rupture, the only course which it is able to take.

This condition of sudden rupture of the gall bladder is not by any means to be confounded with the perforation or slow leakage of a

* Gall Stones and Diseases of the Bile Ducts.

† Gall Stones and their Surgical Treatment.

‡ *Lancet*, 7th April, 1894.

thickened, distended, septic phlegmonous gall bladder, it is indeed exactly the opposite, a phlegmonous bladder often leaks from septic softening and pressure of stones, it always has adhesions to the neighbouring organs or to omentum. If it adheres to the liver it often sheds its stones, forming a liver abscess; but, as far as I know, never ruptures in the acute sudden manner which I am trying to describe.

Symptoms and Physical Signs of Ruptured Gall Bladder.—The patient will usually give a history of repeated gall-stone colic, and will very frequently have suffered from time to time with more or less persistent jaundice; this of course due to the presence of the common bile duct stone. During the course of one of these attacks, he will complain of a dull, heavy pain under his liver, in which situation some swelling will gradually appear; this swelling is absolutely dull on percussion, and it is very tender to the touch. He has troublesome cough, obstinate constipation, and apart from the dull area described above, his abdomen becomes increasingly tympanitic. As times goes on, he vomits and becomes collapsed. The diagnosis on the whole is not hard to make. The signs that stand out prominently are the rapidly increasing dullness under the liver, and the protruding tympanitic area over the left side of the lower abdomen.

The treatment, of course, is early resort to abdominal section. At the first, at any rate, the temperature is low, and as escaped bile is almost sterile, and if cases are operated upon early they should, I think, give a very low mortality, in spite of the urgency of the symptoms and the collapse of the patient.

Record of Case.—E. P., a retired accountant, aged 70. He complained of "gall stones." Two days before I saw him, after a heavy meal, and during a period of constipation, he was suddenly seized with abdominal pain, which from his former experiences he ascribed "to the passage of a gall stone." He had suffered from repeated attacks of the same kind, the acute pain lasting a few hours, coming on and ceasing suddenly, and followed by more or less jaundice, and necessitating rest for a few days. On this occasion, he sought medical advice because his pains and jaundice, instead of abating, were gradually growing worse. I found him in bed with well-marked, but not deep jaundice, a distended abdomen, some rales in the bases of both lungs, and a certain amount of dyspnoea which made it difficult for him to lie down. His pulse was 80, full and regular, and his temperature 97. His abdomen on examination was greatly distended, dull on the right side under his liver, and tympanitic over the left half. I ordered him hot fomentos to his abdomen, a purge of Carlsbad Salts, and a hypodermic of morphine if his pain became worse. Next morning I found that he had passed a restless night, there had been no action of the bowels, and his pulse and temperature were both raised. His abdomen was more distended, and the physical signs before noted were more marked. He had vomited several times and was collapsed. From my examination, I thought it was either a case of ruptured gall bladder, or one of intestinal obstruction from blockage by a large gall stone. He was sent into a private hospital for operation.

Operation.—His abdomen was very greatly distended, and when under ether, two separate bulgings could be very plainly made out. The more obvious of the two was on the left side, and low down, it appeared just like a left ovarian cyst, tympanitic all over on percussion. The swelling on the right under the liver was just as tense, but uniformly dull on percussion. Having raised the end of the operating table, Mayo-Robson's

incision was made running along the costal margin and then directly downwards through the middle of the right rectus. On opening the peritoneum, a jet of bile squirted up as high as the electric light and covered my assistants' and my own head. This was allowed to drain away, and amounted to a great many pints. It was non-odorous, yellowish greenish bile, perfectly clear. As it drained away, the ascending and transverse colon appeared in the wound blanched and ex-sanguine from pressure, and stained with light-coloured bile. The remains of the bile were mopped up, and the gall bladder was found small and cicatrical, tucked well under the liver, to which it was densely adherent, it had a triangular rent with the flap outwards, which occupied the whole of the extremity of the fundus. On tracing the bile duct down, a stone was found in the common duct. This could not be moved, so, after putting two stitches, one on each side, the common duct was incised and the stone removed. The hole in the common duct was then sutured first by tying the ends of the guide sutures together, and afterwards by a few supplementary sutures of fine catgut. After that, a complete search was made for any stones that might be lying free in the peritoneal cavity; none, however, could be found. A cigarette drain was put down to the sutured hole in the common duct, and a large drain into the gall bladder; no posterior drain was inserted.

The patient's condition after the operation was judged to be better than before it was begun. The case went on uninterruptedly well; the cigarette drain was removed on the third day, as there was no leakage of the common duct. The drain in the gall bladder came away on the sixth day, and the sinus leading into the gall bladder closed in sixteen days, and the man left the hospital in three weeks and two days. His cough had given him a good deal of trouble during convalescence.

There is no doubt that the success of this case depended upon the fact that the operation was done before the bile had time to become at least actively septic. Doubtless, in a short time, the bilious fluid would have become infected with the bacillus coli communis, and the case would have developed a rapid septicæmia. There were no definite signs which indicated the exact time at which the gall bladder burst; but, thinking the case over, it is probable that it had happened before I saw the patient. The dullness under the liver, combined with tenderness, which I attributed to omental adhesions round the gall bladder, was no doubt due to distension of the bile trough from a leaking ruptured gall bladder. Moreover, the very large collection of bile, together with the great pressure of fluid, make it almost impossible that the leak could have been one of hours. Moreover, there must have been some adhesion in the lower right abdomen to confine the bile, and we know that adhesions strong enough to stand any real strain will not form very quickly.

DISCUSSION ON AFFECTIONS OF BILIARY PASSAGES.

DR. FIASCHI, SENIOR (Sydney) said: Dr. Hinder has raised the question of cholecystectomy and cholecystostomy. About four years ago, an article appeared in the *Annals of Surgery*, by Dr. Lilienthal, of Mount Sinai Hospital, in which the writer proved himself to be a strong advocate of cholecystectomy, and gave a description of the method employed in 45 cases with the greatest success.

Within the last fourteen years I have performed fourteen cholecystectomies, with one death, and seventeen cholecystostomies without any deaths.

I do not attribute the death to the operation of cholecystectomy, but rather to defective drainage, which led to peritonitis in a few days.

Of the thirteen recoveries from cholecystectomy, none have had fistula. One case, three months after operation, developed symptoms of obstructive gall stones in the common bile duct. In this case I performed a second operation, and removed a calculus from the common duct. In this case, at the first operation I departed from the usual rule of examining all the bile passage before removing the gall-bladder. Whenever the gall bladder is thickened and contracted, it is better to remove it, but otherwise cholecystostomy is preferable. As regards the paper by Dr. Giles, I would suggest one or two points on technique. Where it is difficult to expose the bile passages the falciform ligament may be divided; it exposes the field of operation; the ligament is stitched afterwards. As regards drainage, whenever you open the common duct, you should drain it. The tube should be fixed to the duct with catgut. I frequently suture the common duct, and have never had stricture. I also give urotropine, and have never seen any unpleasant urinary symptoms, which is a big factor.

DR. H. L. MAITLAND (Sydney).—In relation to the incision, my practice has been not to use, as a general rule, the incision Dr. Giles advocates and the one that Mayo Robson advocates. The incision I use is the sub-costal one. This incision, to my mind, is far preferable to any other. In the first place, it gives a better view of the gall bladder and the bile duct than any other incision. Another advantage is there is no division of the nerves. There is less likelihood of hernia afterwards. In the incision through the rectus you are apt to divide the nerves, and so cut the innervation of the rectus muscle, which is more frequently the cause of hernia than the incision of the muscle itself. Dr. Giles spoke of a case of gall stones in the common duct, which was blocked. These can usually be milked along higher up the duct, but it has been my misfortune, in the only case where a stone was blocked, to have it immovably fixed, and I had to do the operation of going through the duodenum, and so extracting the stone. Acute pancreatitis is also mentioned in both papers, and Dr. Hinder said that he had only had two cases in the records of the last two or three years. I do not know how many we have had in the Sydney Hospital. I myself have had two cases. One very interesting case occurred some five or six years ago. I must confess I had made no diagnosis before operating. The case—a nurse—was transferred to me, and I thought it was intestinal obstruction. On opening the abdomen it was seen to be a case of acute pancreatitis. I simply opened the abdomen, and ran the knife along the pancreas, which was contused and swollen. I drained the abdomen, and she got well. In the second case, I drained, and the patient died. Dr. Hinder's view as to the cause of pain in gall stone diseases, to a very great extent, is probably right. He also mentions the difficulty of diagnosing between gall-stone diseases and appendicitis that sometimes occurs, and I remember having a case of acute appendicitis, where the appendix was adhering to the gall bladder, forming an abscess and infecting the gall bladder, showing that you may get a case of appendicitis which absolutely resembles cholangitis. In regard to the question which has been raised between cholecystectomy and cholecystostomy, I entirely agree with Dr. Hinder in his views. Cholecystectomy is an ideal operation. It gets rid of the stones and removes all likelihood of their reformation, but I think it is

more than outweighed by the greater risk. In a simple case of a moderately healthy gall bladder filled with stones, provided you are quite sure the common duct is empty, then you may remove the gall bladder, but in the vast majority of cases I think the main indication is adequate and complete drainage. It is far more important to my mind, and gives a more favorable prognosis than cholecystectomy.

DR. DUNCAN (Kyneton).—I desire to emphasise one or two matters. Preparatory to operating on a patient of this kind, the surgeon should be quite sure that the operating region is thrown sufficiently forward by the sand-bag. This will make the operation very much easier, and bring about the partial dislocation of the liver, and allow it to be rotated; then the gall bladder and bile ducts are fully exposed. I must confess that I do not care for the sub-costal incision. I think that favoured by Bevan and Mayo Robson is preferable. I think, if care is taken with this incision, the operation is greatly facilitated. In regard to the question of drainage, my experience is against gauze drainage; gauze can be used inside a large tube and removed as often as required.

DR. COOKE (Melbourne).—After twenty years' experience, I would say that previously I used to sew up the gall bladder after the stones were removed. Most of these cases did well; there would be a rise of temperature for two or three days, and then it subsided, and usually the patient would go out in about two or three weeks, the ultimate result being perfect. I still pursue this treatment as long as there is no suppuration about the gall bladder, but I insert a drain through the parietes to drain any leakage of bile. In 48 hours, I withdraw the tube. Previous to this, at the operation, I thoroughly wash away all purulent secretion.

DR. BOWKER (Sydney).—Dr. Hinder has stated that he did not consider the effort to get rid of stones was really the cause of biliary colic. I think there is but little doubt that biliary colic is caused by the efforts of the ducts to get rid of stones. Having tried to get rid of them, and not having done so, the gall bladder becomes distended, and there is a feeling of tenderness constantly there. In regard to large gall stones, it is curious at times to see that large stones may have been present for a long time without causing any symptoms whatever. I remember seeing a case in Sydney where there was great difficulty of diagnosing between gall-stone pain and pain of appendicitis. This young woman had been seized suddenly with a tremendous pain in the right region, and a fellow surgeon and myself both thought it was an acute attack of appendicitis, in fact, so much so that I cut down on her appendix and removed it. Then I cut a little higher, down to the gall bladder, and came across a sausage-shaped tumor with a huge stone, and the gall bladder almost ruptured. The patient got perfectly well. In differentiating between the pain of gall stone and the other pain there is one point which has not been mentioned by the writers of these two papers, that is, pressing the ribs together. I have found it valuable on various occasions. With regard to cholecystectomy, I certainly agree with Dr. Hinder. As regards cholecystectomy, I have never performed the operation, and I never intend, if possible, to do so. With regard to acute pancreatitis, just about the time they began to write upon this disease I came across a case which I did not recognise at the time. I think we had half the profession in Sydney to see it. At the suggestion of one surgeon, I removed the appendix, and at the suggestion of another I removed the gall bladder. Eventually the man died. In regard to drainage of the gall duct, I always drain the gall duct. I might stitch it up and make it a little smaller, and then stitch the drainage tube in with

plain gut. In regard to urotropine, no doubt it cannot do much harm, but I do not know that it can do a great deal of good. The practice which I constantly follow out is the giving of large doses of chloride of calcium. With regard to the question of incision, I dislike large incisions in the abdominal wall. Unnecessarily large incisions, I think, are terrible things. In most of the gall-bladder surgery I have done, I do not think I have had to enlarge an ordinary straight incision. If it is made low enough down, and with the body supported with a pillow, we can generally get at the gall bladder, particularly if we extend the incision a little lower.

DR. BIRD (Melbourne).—It is said that one cannot get gall stones again if the bladder is removed; that is not so. I am not in favour of cholecystectomy, except when it is absolutely necessary. Personally, I do not care for a very long incision. I like a fair-sized incision. If you make it too far down, you get into the small intestinal area, which you want to avoid. If I want more room, I prefer to run up along the ribs rather than go down into the small intestinal region. I very seldom drain, I find I get on better without it. As a rule, I put sutures in the common duct.

DR. MOORE (Melbourne).—With regard to the question of the cause of pain in these particular cases, it seems to me it must be very different in different cases, and it must be quite impossible to fix one cause. An interesting case which occurred recently seems worthy of mention, and that was a patient from whom I had removed gall stones several years previously. Again she was having repeated attacks of what was apparently gall-stone colic. She had terrible attacks of pain, but there was never any trace of jaundice. I re-opened the old incision and was able to free the adhesions and to remove the gall bladder, which contained gall stones. These stones were very small and very sharp edged, and it certainly seems to me that the patient's attacks of pain were caused by the passage of one of these sharp-edged stones down the common duct, but the stones being so small, and being particularly sharp, they did not cause obstruction at all. That case certainly would seem to indicate that in some cases the pain is really due to the passage of the stone along the duct. I have seen one or two other cases where stones passed away, fairly small and sharp-edged, and where the pain was very severe. Another case recently, which indicated altogether a different cause of pain, was in an elderly woman, who had repeated attacks of pain, which always started at the left side, and radiated to the right side. She came into the hospital with an attack which commenced in the same way, but on this occasion she had a lump in the region of the gall bladder. There was a rise of temperature and empyema of the gall bladder. There was a very large stone, shaped something like the thumb, blocking the cystic duct. There the cause of the pain was different, and, as I supposed, associated with the presence of this stone in the cystic duct. With regard to incisions, I have always preferred the sub-costal incision, as recommended by Kocher. There are objections to it. The worst objection is the great difficulty of suturing correctly. The muscles run in different directions, and you have some separating much more widely than others, and if you suture in layers it is a long process, and sometimes operations are prolonged, and you do not wish them to be prolonged further by elaborate suturing. That seems to be the worst objection. No doubt its advantages are that you get at the parts better. You do not go down into the region of the abdomen, where you have the small intestines, and you do not divide the nerves to the same extent as you do with a long incision down the rectus. I do not think there is much in the risk of hernia. I think the abdominal wall is considerably weakened if you paralyze a great

portion of the rectus. In regard to what I consider a most important matter—the question of the removal of the gall bladder—I would like to know why you should not remove the gall bladder. I do not see why you are more liable to get stones in the common duct than when you do not remove the gall bladder. I can easily understand that jaundice might be more troublesome to deal with if you did get it when you have removed the gall bladder, but I certainly think the liability would be rather less when you lose the gall bladder than when you had it. About the only occasion when I would say I would not be inclined to remove the gall bladder would be if I was doubtful about the common duct being clear. In a great many operations for gall stones, there is no doubt of gall stones being there, because you have not got jaundice. If there is no jaundice, the common duct is not blocked. At any rate, I think in each class of case you are perfectly safe in removing the gall bladder. I do not know any use the gall bladder is, unless for collecting stones, therefore, I cannot see why the gall bladder cannot be removed. In the case I mentioned with regard to pain, where the patient had distended gall bladder, I emptied the gall bladder, and as soon as I emptied it I clamped the opening and went straight on to remove the gall bladder, and when I got the gall bladder drawn up, I found a large stone in the cystic duct, and then I closed over the whole of the wound with peritoneum. In the last six months I have operated on four or five cases in elderly people. In four cases, they had reached over 60 years of age, and one of them was 73, and that patient had a large stone in the common duct. I removed the gall bladder also. With the other patients, all over 60, and by no means healthy, the whole of them recovered, and rapidly at that. It seems to me that the recovery after the removal of the gall bladder is much more rapid, and very much more satisfactory than it is in the cases where you drain, and, for that reason, in cases where the gall bladder is at all thickened and I am sure the common duct is open, I have no hesitation in removing it. The case I have mentioned of the small gall stones is the only one I can remember where the gall bladder had been emptied, and there was no good reason to believe that the stone had been left behind when the gall stones reformed. As a matter of fact, the gall stones were far more numerous on the second occasion. Still, it is one case where gall stones had reformed. That is another reason why the gall bladder should be removed. I am surprised that Dr. Hinder does not remove the gall bladder. On two occasions I had patients die from malignant gall bladder after I had removed the stones. That is another reason why I advocate the removal of the gall bladder. The third reason is that I do not see any reason why it should not be removed. I do not see any use in it. In many cases the operation is very little more difficult than the simple removal of the gall stones. When the operation is finished, it is much more satisfactory than the drainage of the bladder.

DR. JACKSON (Brisbane).—I have had some little experience of gall-stone surgery, and I agree with the gentlemen who have advocated the "S"-shaped incision. I have found that generally easy myself. Drainage I use whenever I get any leakage. With regard to the removal of the gall bladder; I never remove it unless I see the most absolute necessity for it. I removed a gall bladder recently, when all the stones were tightly bound up. I satisfied myself that the common duct was free, and I removed it, with good results.

DR. RAMSAY (Launceston).—The only point I would like to draw attention to is cholecystectomy. I consider that in cases where the gall bladder is obviously diseased that it has no special important function

to fulfil, and, as Dr. Moore has said, it is an organ we can very well spare, and if the objection is raised to the operation that we may get stones formed in the ducts, surely then we ought to tackle such stones in the ordinary way we tackle the stones in the common duct. The only objection I can see is that it may be a little difficult in obtaining access. In regard to drainage, personally I think that when the common duct is widely dilated we can well afford to run the risk of stricture, because we have plenty to draw upon. I consider that in many cases we can well sew up the duct without putting a drainage tube into it. On the other hand, when we find an intensely infected common duct, I think it is our duty to find an outward drainage. A case is running in my mind of septic cholangitis, where one simply entered the drainage tube into the duct, and in a day half an ounce of pus came out.

DR. SYME (Melbourne).—With regard to incisions, I have tried all, and generally I have used Moynihan's. I prefer to make it almost central and retract the rectus to the right side. The objection to the sub-costal incision is the difficulty of obtaining accuracy in closure. With the other one, you interpose the rectus back again. This prevents hernia. On the whole, I prefer the incision I have frequently used to the sub-costal incision, but I use the latter when the wall is very thick and I anticipate much difficulty in getting at the liver. I prefer it more in men than in women. The question of cholecystostomy *versus* cholecystectomy is an important one. I have been a great advocate of cholecystectomy. I do not think the gall bladder is of much use. The majority of stones do form in the gall bladder, and I have seen two cases in which the gall bladder had been opened and freed of stones, and the ducts proved thoroughly clear of stones, but a second operation was performed, and some small stones found. These stones had formed in the bladder since the previous operation. It is only in a diseased condition of gall bladder that stone forms, as the result of the infection of the mucous membrane. If the gall bladder is diseased, I think it is better to remove it. If a stone is blocked in the cystic duct, I would remove the gall bladder. If I feel a stone in the cystic duct, I immediately remove the gall bladder and stone, without opening the gall bladder. On the other hand, you may get a very adherent and thick gall bladder, the very one you want to remove; but it may be a very difficult operation. I have had some cases which have made me question the advisability of performing cholecystectomy so universally. One was a case in which stones kept on coming down the hepatic duct, dozens of them, after operation. I took them out almost every dressing. If the stones can go on forming in that manner and come away, they may get down and block the common duct, and then if you have removed the gall bladder, you may get into trouble. There are cases where obstructions may be due, not to stone in the common duct, but to stricture. Then, again, there are cases where the operation is so complicated that it may be safer not to do a thing that you may regard as ideal, but to do what you consider quickest and safest. I had hoped that the discussion would have led us to some means of discriminating as to when it is best to do one operation or another. No doubt in a very large number of cases cholecystostomy would do perfectly well. In all cases you must explore the ducts, and you must satisfy yourself absolutely of the patency of the ducts. Personally, I am not satisfied until I have done that. It is wonderful how small a stone will cause great trouble. I explored in one case, and could not feel any stone. At last I thought I felt something, and opened the common duct. There was a very small

sharp stone impacted there, and yet it was causing very severe symptoms, and was very difficult to detect. In the same way, I discovered a very small stone in the cystic duct. In another case I found a relatively small stone in the ampulla, just at the end of the operation, when I had almost given up the investigation.

DR. GILES (Adelaide).—So far as the question of the best manner of making the incision is concerned, Dr. Maitland prefers one method, and I prefer another, and I suppose it all depends upon what we are accustomed to. I do not think it is a wise thing to prolong the incision downwards. The object is attained if a very long straight incision is made, but if more access is required, the incision should be prolonged upwards, and then rotate the liver, so that you get thorough access, and there is less danger of hernia. I am very strongly impressed with the importance of free drainage, and I think I emphasized that in my paper. The drainage tubes I always fix in the duct by means of catgut. Cholecystectomy, I know, is strongly advocated by many surgeons. I have done it on several occasions without apparently special difficulty, but I do not believe in it as a routine practice. American surgeons generally agree that it should be done. There was a majority in favour of this; it was generally adopted on account of the danger of cancer. I think that we are now waking up to the fact that cancer of the gall bladder does occur in these cases very much more frequently than we originally imagined. We now know, too, the importance of establishing permanent drainage. I had some remarks to make in regard to urotropine. I read Dr. Fiaschi's paper in the *Australasian Medical Gazette*, and I have determined to give it a trial.

DR. HINDER.—In the first place, I support cholecystostomy, for the reason that I never yet had a patient die of malignant disease of the gall bladder after operation. I never had to operate on a patient a second time for stones in the gall bladder, except on one occasion, when the patient's gall bladder was divided into two parts. On the second operation it was found that there was a small stone at the side of the gall bladder. It is said by some writers that the removal of the gall bladder is a good thing. We do not know positively the function of the gall bladder, but we know that it keeps up a steady even pressure on the bile ducts. It is found that the common duct is dilated because it is attempting to take on the function of the gall bladder. I would be very glad to see a few men keep on removing the gall bladder, because unless that is done, none of us will know for a few years whether cholecystectomy is the right thing. I will keep on doing what I have done. If we look at the statistics of competent authorities, or men who operate on a considerable number of cases, we find that, even in their hands, the mortality after the removal of the gall bladder is greater than it is from simple drainage, so I do not see any points from a surgical view to be gained in removing from a patient more than is absolutely necessary. These are really my main reasons, and I think I will continue to do it until evidence of a few years is brought forward to show, at all events, that my custom is a faulty one, or that the custom being followed is considerably better. Speaking to Dr. MacCormick a few weeks ago, he told me that in operating for stone in the bile duct he found the stone pressed down against the duodenum. He opened up the duodenum and removed the stone, and it was then agreed that that would be a much better method to remove the stone. I said that if the stone was removed through a thin duct, the stitching of the duct was a rather difficult thing, and one which you could

not conduct in a correct fashion, whereas every time you had a stone in the duct you could get it out by pushing it into the gall bladder, or get it out through the duodenum in that way. After taking the stone from the duodenum, there was a big opening, and all the rubbish flowed away into the duodenum. There was absolutely no reason why you could not shut up the incision without draining the common duct at all.

THE SURGERY OF NON-MALIGNANT DISEASE OF THE STOMACH.

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Amongst diseases not due to cancer capable of surgical treatment, none are more important than those affecting the stomach, for, besides causing pain, they soon impair our nutrition, and are rapidly followed by emaciation, weakness, and disablement. Principal amongst these is gastric ulcer, a painful and dangerous complaint. The 27th September, 1881, is the memorable date when surgeons first attempted to treat it by operation. On that day, Wolfer, under the inspiration of Nicoladoni, performed his first anterior gastro-enterostomy. Their idea was to form a new opening between the stomach and intestine, so as to deviate the current of acid chymus from the pyloric end, where ulcers are most commonly located. The first attempts of Wolfer and his followers were attended by many cruel disappointments, due partly to defects in technique, such as bad stitching or sloughing through of Murphy's button, and partly to an inherent defect of the method which frequently causes regurgitant vomiting. It was not till after 1900 that real progress was made, when Von Hacker passed the jejunum through a rent in the transverse mesocolon and attached it to the posterior wall of the stomach, and Von Czerny and Petersen had the ingenious idea of anastomosing the jejunum to the stomach short, that is, 2 to 4 inches from the duodenum.

The introduction of this "no-loop" method immediately caused a great diminution of mortality, and with the improvements in the smaller details of the technique brought in by Mayo Robson, Moynihan, William and Charles Mayo, and others, the statistics so improved that the operation could be considered a reasonably safe one. The operation perfected, the problem followed, when should it be applied? During the last eight years there has been much groping about to solve it, and let us admit it, there has been abuse or improper application of the operation. But now I believe we are getting to see more clearly which are the cases of gastric ulcer that require a gastro-jejunostomy, and which are those in which it should not be performed.

The best authorities, whether physicians or surgeons, are agreed that the so-called acute-gastric ulcer, the one more commonly found in the young, is in the majority of cases curable by medical treatment. These should not be touched by the surgeon unless such complications as arterial hæmorrhage or perforation occur, or unless after a prolonged period of dietetic and medicinal treatment no improvement follows, and the patient is so reduced in strength and flesh as to be disabled from taking active part in the duties of life.

On the other hand, the chronic ulcer, more common in middle life, generally attended by much thickening around it, and very often accompanied by cicatricial retraction, causing either alteration in the position of the pylorus or actual stenosis of it, with subsequent dilatation of the stomach, should receive the benefit of a gastro-jejunostomy. These are the cases where the results are very gratifying. I have heard patients of this class remark—"I have suffered with my stomach for twenty years, and been to twenty doctors, without ever getting the slightest relief. The operation has cured me of all my pain and vomiting, and I am a new man." To further prolong the sufferings and disablement of this class of patients by a vain endeavour to cure them with medicinal or dietetic treatment is a dangerous mistake.

Alongside these two well-known types of gastric ulcer there is a large number of cases in which the clinical symptoms of gastric ulcer are present, but the ulcer is not there. There may be pyloric spasm, a reflex from gall stones, or some morbid condition of the intestine, such as appendicitis and tubercular disease, or it may be a pure neurotic mimicry. On the existence of the latter there is much difference of opinion, both amongst physicians and surgeons, some thinking that gastric neurosis is due to some condition that we do not understand. For practical purposes, we must recognise this class of cases as existent, and that it is the most unpleasant you may have to deal with.

Now it is fair in all these cases, when medical treatment has been inefficient, to do an exploratory section, and deal with the gall stones, or the appendicitis, or the adhesions, if found. But be on your guard, unless you find such evidence of gastric ulcer, or duodenal ulcer, or stenosis of the pylorus capable, as William Mayo states, of being demonstrated to a spectator standing 6 or 7 feet from the operating table, do not perform a gastro-enterostomy. Naturally, now we must learn to find where these ulcers are, and in that, again, the Mayos have greatly assisted us by establishing from their large operative experience the fact that no less than 40 per cent. of all gastric and duodenal ulcers are situated in the duodenum.*

For the future, if you wish to be successful gastric surgeons, you must acquire this ability of being able to detect on opening the abdomen any abnormal condition that may be present, and to decide if further surgical interference is required or not. Hence in these cases the opening of the abdomen is a solemn moment, and the surgeon should then have all his faculties on the alert, and pause before coming to the important decision.

When, after due observation, you conclude that the case comes within the class of the neurotics, do not proceed any further, close the abdomen. I may mention two of my cases in contrast. One, a well-nourished young man, who came from the country to the Sydney Hospital with an escort of sympathising friends, and who gave me an account presenting all the complex symptoms of gastric ulcer. His tale of woe was so pitiful that it biassed me, and his appeal for relief was so urgent that I operated on him. On opening the abdomen, the examination of the stomach and duodenum above and below revealed no abnormal condition, but I was so convinced of his having a gastric ulcer that I disbelieved my senses, and performed a gastro-jejunostomy. The lad had hardly recovered from the anæsthetic when he called for the resident surgeon and told him that he was afraid the operation would do him no good, and that he was sorry that he had stated some untruths about himself and his complaint.

*Mütter Lecture—December, 1906—Philadelphia, as quoted in *Progressive Medicine*, June, 1907, page 72.

He made a good operative recovery, and was able to eat well, and to continue in his good physical condition, but his incessant complaints did not abate one iota, and before he left hospital he had developed one or two mimicry diseases of other organs.

The other case was a young girl who had for four years been subject to incessant vomiting. Everything she took came up; at the same time, though thin, there was not emaciation proportioned to her vomiting. I made an exploratory incision, found no evidence of organic disease of the stomach, and I closed the wound, but during the convalescence I carried out the suggestion of Willy Meyer,[†] I briefly informed the patient that the cause of vomiting had been removed and that she would not vomit again, at the same time I cautioned all the nurses to say no more. She never vomited since, made a splendid recovery, and keeps well.

This plan will to you be repulsive, as playing with truth, and it was so with me, but I give it to you as I have received it from Meyer, and the result has unquestionably been all that could be wished.

I here present you my list of gastro-jejunostomies performed in the last six years. You will see that I had four deaths, of which two are due to defects of technique, and two to bad luck. You will notice the great improvement after 1906. It corresponds with my complete adoption of the technique and method of W. Mayo, which I have followed in its entirety, with the jejunum running to the left. As regards remote results, with the exception of the hypochondriac who remained unrelieved, all the others have done very well; no vomiting nor peptic ulcer followed.

Perforation.—One of the complications of gastric ulcer demanding immediate recognition and operation is perforation. Within the last two years I have operated on three perforations, two for duodenal, and one for gastric ulcer. The first duodenal had been perforated five hours; I sutured it, but I committed the error of doing a gastro-jejunostomy. The patient died on the third day. At the autopsy both the ulcer and the gastro-jejunostomy were found absolutely sealed, so we concluded that death had occurred partly from shock and partly from peritonitis. The second duodenal ulcer had perforated six hours before. I simply sutured it and patient made a good recovery, not having suffered from symptoms of gastric ulcer since. The third case, a gastric ulcer, had been perforated 26 hours. On opening the abdomen I found a large callous ulcer on the upper part of the lesser curvature just close to the cardia. It was in a most inaccessible spot, and the margins were so friable that the sutures did not hold. I managed by taking a wide sweep to infold the ulcer with gastric serosa and with portion of the gastro-hepatic ligament, but as on pressing on the stomach there appeared to be a little leakage, I plugged with two or three yards of sterilised gauze and left a drainage tube. I also put a tube into Douglas' pouch, which was already full of turbid fluid. I did not wash out the upper peritoneum, but flushed out with salt solution Douglas' pouch. For a week the plug was left in, then gradually removed. There has been no leakage of food, though there has been suppuration, and the girl appeared to be making a good recovery. Patient died a month later from effects of sub-phrenic abscess which was recognised and opened, but too late.

The general consensus of opinion now is that in cases of perforated gastric ulcer the surgeon should limit himself to stitch up and close the perforation, no gastro-jejunostomy should be performed. Such view is

[†] The Operative Treatment of Intractable Vomiting, not due to Pyloric Obstruction, by Willy Meyer, *Annals of Surgery*, May, 1908, page 730.

corroborated by W. Mayo's opinion that when gastric ulcers perforate, if stitched up they heal rapidly and cause no further trouble. W. Mayo has been so impressed by this fact† that when dealing "with chronic gastric ulcers, where the conditions were such that he could not excise them, and where gastro-jejunostomy was not indicated, with a sharp knife he cut out the base of the ulcer, thus producing the picture of acute perforation, then closed the defect. This has been done with favorable results."

Hæmorrhage.—Of hæmatemesis due to gastric ulcer I have had no surgical experience. So far I have adopted only the usual medical treatment, rest, ice, and morphine.

Rodman says§ that in the large hæmorrhage which accompanies "acute ulcer of the stomach, surgical intervention is wholly unnecessary. Of all the remedies employed for arresting such hæmatemesis, none act so promptly and so certainly as hot water (temperature 120 dgres to 130 degrees) introduced through a stomach tube. The hæmorrhage in chronic ulcer is generally arterial, and is recurrent. After the second attack, operation should be performed. The stomach should be opened, and a direct search made for the bleeding vessel with a speculum and electric light. If it can be found, the ulcer should be excised, or a partial gastrectomy performed. The next best method is the ligation of the mucous membrane *en masse*, so as to draw a cone into the stomach. Transfixion of the bleeding surface and ligation have also succeeded. Cauterisation is theoretically unsafe, and has in several cases caused perforation. Should the search for the bleeding point prove futile, the opening in the stomach may be utilized for gastro-enterostomy. Rodman does not advocate this procedure for hæmorrhage except as a last resort. That it will relieve capillary bleeding is likely, that it will arrest hæmorrhage from a vessel of considerable size is questionable."

Inflammatory Affections of the Stomach.—From an old and well-known type of disease, gastric ulcer, I shall pass now to a less known and obscure affection, an inflammation affecting chiefly the areolar tissue surrounding the vessels of the stomach, which may sometimes pass to suppuration and cause abscess in the wall of the stomach, other times merely cause a fibrinous infiltration which will lead to great thickening of the walls of the stomach and to contraction, I may say almost to obliteration of its cavity. This form of disease has received various names, suppurative phlegmonous gastritis, obliteration of the stomach, and linitis plastica and suppurativa. I prefer the latter term, which is derived from *λινον* web and *ιτις* inflammation. It was first used by Brinton in his book, *Lectures on Diseases of the Stomach*, published in London in 1864. Though known to physicians, it is only but recently that surgeons have ventured to deal with these conditions. One very fine case is recorded of linitis suppurativa, in which the abdomen was opened and an abscess found within the walls of the stomach near the pylorus. This was incised into, the very thick pus expressed out, and a drainage tube left in. Recovery followed. The surgeon was Bovée, of Washington,|| and this is the first case recorded.

Of the other variety, linitis plastica, there are two very fine cases published. The first one is by Professor Von Eiselsberg, of Vienna.¶

† *Annals of Surgery*, June, 1908, page 887.

§ *Journal of the American Medical Association*, 1906, Vol. XLVII., page 142, quoted by E. M. Foote, *Progressive Medicine*, June, 1907, page 83.

|| Bovée. Suppurative phlegmonous gastritis. *The American Journal of Medical Sciences*, May, 1902, page 662.

¶ *Surgery, Gynaecology, and Obstetrics*, August, 1908, page 254.

The patient, a middle-aged woman, suffered from emaciation, sensation of pressure in the pit of the stomach, and vomiting. The stomach was found surrounded with adhesions. These were separated, and eighteen days after the abdomen was re-opened and the stomach found to be a narrow tube with walls from half to one inch thick. As there was not sufficient normal tissue present, it was found impossible to perform gastro-jejunostomy, and a jejunostomy according to Witzel's method was done. The patient made a good recovery, regained her weight, and lived, nourishing herself partly *per orem*, partly *per fistulam*. The second case is by Dr. James B. Bullit,** of Louisville, Kentucky, and a very similar condition to the above was met. A thickened narrow tubular-like stomach was found buried in adhesions. There was no possibility for a gastro-enterostomy, so a duodenostomy was performed and the patient did well.

I think these two cases teach us that when linitis plastica is found, we should not hesitate to establish a stoma below the stomach either in the duodenum or the jejunum.

Adhesions.—It is an easy step from intrinsic inflammation of the stomach to pass to those perigastric inflammations which leave behind adhesions. These are very frequently the result of operations done for other conditions, such as gall stones, or of injuries, such as wounds and lacerations of the liver, or abscesses of that organ. These adhesions are apt to alter the normal position of the stomach and draw on the pylorus, thus causing stenosis. The duty of the surgeon when finding such condition on an exploratory incision is to separate all the adhesions and to use his art so that no further adhesions may be formed in the future. Scrupulous asepsis and attention to cover all raw surfaces with peritoneum, what the French call peritonisation, will help to avoid them. As a preventive the plan suggested and tried successfully by J. B. Blake†† of pouring sterile oil on the parts likely to adhere may be adopted. This oil is thoroughly sterilized by boiling for one hour, and from one to three drachms are used.

In speaking of adhesions one should not forget prevention, if possible. It is a good thing after any serious operation on the gall-bladder to prevent the formation of omental adhesions, which will ultimately drag on the duodenum or pylorus, by a method of colo-hepatopexy. This consists in the attachment of the transverse colon to the edge of the liver, thus putting the colon as a screen between the stomach and the omentum.

Omental adhesions following an appendectomy or salpingectomy may frequently cause ptosis of the stomach and of the transverse colon. When such condition is found, the adhesions should be separated, and the stomach attached, by Beyer's operation, which consists in the shortening of the gastric ligaments, or by Coffey's operation, in which both stomach and transverse colon are elevated, and the omentum attached to the anterior wall of the abdomen. Much information has been gained recently on the subject of ptosis of the stomach and colon, by Dr. Clark,‡‡ of Philadelphia, who has radiographed patients after the injection of bismuth. His radiograms are very instructive, and I think that no attempt to operate for ptosis should now be made unless a previous satisfactory Rontgenogram has been obtained. Even then much judgment is required

** Bullit Obliteration of the Stomach as a result of Gastric Ulcer. Duodenostomy. *Annals of Surgery*, January, 1907.

†† John Baptist Blake. The Use of Sterile Oil to prevent intra-Peritoneal Adhesions. *Surgery, Gynecology, and Obstetrics*, June, 1900, page 667.

‡‡ John G. Olark. Surgical Phases of Enteroptosis. *Surgery, Gynecology, and Obstetrics*. April, 1908, page 339.

lest this class of operation may be used in neurotics and in unsuitable cases, as we know has happened too often in the surgery of movable kidneys.

Congenital hypertrophic stenosis of the pylorus is a very interesting infantile disease recently studied, and about which it has not yet been decided whether it is a medical or surgical disease. First described by Beardsley and Williamson in 1841, it has since been carefully studied by Cautley, Thompson, Stiles, Koplik, Burghard, Nicholls and others. It occurs in infants, generally boys, whether fed by artificial foods or by the breast. The cardinal symptoms are vomiting and wasting, with visible exaggerated peristaltic waves going in the epigastrium from left to right, and with a well-marked palpable swelling in the region of the pylorus as if this organ became erected. It is due to hypertrophy of the pylorus, causing stenosis. The views as regards treatment vary. Cautley, a physician, §§ considers that "these cases are almost always fatal unless treated by operation; that prolonged medical treatment causes serious loss of valuable time and increases the risk of operative measures, that operation reduces the risk of a fatal issue, and that the ultimate future of the child is more assured."

Koplik, || another physician, the latest writer on the subject, feels that his chances with nature are as good, if not in some cases better, than with the operative measure.

The operations adopted for the treatment of this condition have been Loreta's divulsion, gastro-enterostomy, and pyloroplasty. The mortality from 114 cases collected by Bunts for divulsion have been 51.8 per cent.; gastro-enterostomy, 53.6; pyloroplasty, 53 per cent. As Bunts remarks: "it is rather surprising that an operation seemingly so unsurgical in its method, and so unpromising as to its permanency, as divulsion, should show the most favorable percentage of recoveries."

In Australia, so far, to my knowledge no cases of this kind have been operated and published. Therefore, we must be satisfied with the experience of our colleagues in Europe and in America. Had I a case I would try Nicoll's modified form of pyloroplasty.

Syphilis.—That syphilis may attack the stomach has long been a vague idea of medical men, and who amongst us senior members cannot remember cases where all the symptoms of gastric cancer, including a suspicious swelling at the pylorus, were present; when the discovery of some stigma, characteristic of syphilis, induced us to try an active specific treatment, with the result of complete disappearance of the swelling and of all the other dread gastric symptoms. The idea now has lost all vagueness, and has become an established fact, thanks to the numerous explorations in the lining of the suspected stomach. To none are we more indebted for this, than to a gentleman here present, Dr. Fred. Bird, who, in a very fine article on Syphilis ¶ as a cause of stomach disease, gave us his experiences of such cases, and his advice as to what to do when confronted at the operating table with a syphiloma of the stomach.

If, on opening the abdomen, we find those signs so well described by Dr. Bird, characteristic of visceral syphilis, such as thickening of and opacity of the peritoneum over the spleen (syphilitic perisplenitis), or over the liver, and the bothryoid or bunch of grapes-like appearance of the

§§ *British Medical Journal*, 13th October, 1906. A Discussion on Congenital Pyloric Stenosis.

||| Koplik. Congenital Pyloric Spasm. *The American Journal of the Medical Sciences*, July, 1908, page 1.

¶ *Surgery, Gynaecology, and Obstetrics*. Chicago, May, 1907, page 635.

liver, also the bluish striæ following the course of the lymphatics, or splotches of opaque bluish white on the serosa, or starred cicatrices with strongly fibrous or even calcareous centres, then if there is a pyloric tumour we have reason to suspect that it may be a syphiloma. The differential diagnosis between a pyloric syphiloma and a carcinoma is in many cases so difficult as to be impossible without the microscope; hence Dr. Bird's suggestion, that when in such doubt, we should remove a piece of the growth or of a gland for examination by a pathologist, and, according to the verdict, either do a partial gastrectomy or simply put the patient on active anti-syphilitic treatment.

This plan, sound in theory, is, in my opinion, not always advisable in practice. If the pathologist is at hand, and the examination can be rapidly done before the abdomen is closed, very good. If not, I think that we shall expose ourselves to the risk of leaving many operable gastric cancers to run their fatal course. Patients often after a simple exploratory incision feel temporarily better, and may refuse a second section. Our experience in a much more accessible region, the tongue, has shown in the past, that excessive anxiety to exclude syphilis has frequently caused fatal delay in the treatment of cancer. Nor should we, in these difficult cases, throw all the burden of responsibility on the pathologist.

I agree with Dr. Bird that every attention before and during the operation should be paid to the exclusion of syphilis. As the Roman emperors of old in their triumphal marches had a slave behind them whispering in their ears the warning words: *Respicere post te, hominem memento te*," so let the modern surgeon on opening the abdomen have a censor at his side to whisper the words: *memento syphilidem*. That is right, but if, heeding all these warnings, we still remain in doubt, and the growth has the ominous appearances of cancer. I think that we should promptly remove it.

Tuberculosis.—Of late, various observers, particularly in France, have studied the effects of tuberculosis on the stomach. Poncet,* of Lyons, in his name, and in that of Mr. Leriche, made a communication to the Société de Chirurgie, stating that there exist three great forms of surgical tuberculosis of the stomach: 1st. An ulcerating form; 2nd. An hypertrophic form, a gastric tuberculoma; 3rd. An inflammatory form. As a result of the latter we may have a pyloric stenosis "*d'emblée*," quite distinct from all other forms of gastric stenosis. This is new ground, and these views are yet *sub judice*, but it is right that a progressive body of surgeons as here present should consider them.

I may say that I have a case in support of the ulcerative form of pyloric tuberculosis. This was a publican, aged 29, seen by me in August, 1904. He then had a cough, slight expectoration, night sweats, and wasting. He presented some sub-crepitant râles in the middle of right lung, and though no T.B. could be found in his sputum, he had all the appearances of suffering from pulmonary tuberculosis. His hotel was in a low-lying district, so I ordered him to part with it. This he did, and went to live in a dry healthy locality, passing all his time in the open air and using a liberal diet. All the pulmonary symptoms disappeared, he gained flesh again, and kept well till December, 1907, when gastric symptoms set in. There was great pain in the epigastrium after meals, and vomiting, with loss of flesh and strength. He occasionally vomited mucus and blood. For several months he was dieted and treated medicinally, but as he was

* *La Semaine Médicale*, 3 Juin, 1908, page 273.

rapidly losing flesh, I decided in January, 1908, to operate on him. I failed to find any very characteristic lesion in the stomach, except a certain amount of pyloric stenosis, and on that account I performed a posterior gastro-jejunosomy. He made a good recovery, lost all his gastric symptoms, gained flesh and strength, and seems to have lost all pulmonary and gastric trouble.

The absence of T.B. in this man's sputum may somewhat impair my view that the gastric trouble was due to tubercular ulceration, and he may in reality have suffered from an ordinary gastric ulcer near the pylorus.

Post Operative Duodenal Ileus.—We come now to a very serious condition of the stomach, which is apt to follow abdominal and pelvic operations, and is known under the various names of post operative duodenal ileus, acute dilatation of the stomach, gastro-mesenteric ileus, and post operative gastric paralysis. It is generally accepted now, that the cause of it is strangulation of the duodenum by pressure on it from the root of the mesentery and the superior mesenteric vessels. It is ushered in by nervous restlessness, followed by vomiting, and great distension of the stomach from carbon dioxide gas. It is a very dangerous condition, and the best treatment for it is to use the stomach tube and to avoid dorsal decubitus. Schnitzler, of Vienna,† has treated successfully these cases by making the patients lie on the abdomen for a few minutes. This establishes again the permeability of the intestine, which reveals itself by the escape of flatus per anum and eructations, in consequence of which the meteorism soon disappears. I had this year one case in a woman presenting the symptoms of post operative duodenal ileus, which passed on using the stomach tube, and on following Schnitzler's postural treatment.

Wounds of the Stomach.—In civil practice all penetrating wounds of the stomach, whether from bullet or cold steel, should have immediate laparotomy performed. In such wounds it is rare that the stomach only is wounded. Other organs are generally involved. The associated injuries are usually of graver significance than the gastric injury. Walton Martin,‡ of New York, has given a series of 25 such wounds. The wounds of entrance and of exit in the stomach were closed first by a purse-string suture, and then by reinforcing Lembert sutures of catgut. Two cases, in addition, required splenectomy, two thoracotomy, five suture of the diaphragm, one incision of the 12th rib, one packing of the kidney, and one cholecystectomy. The total mortality was 25 per cent., a great improvement on the non-operative treatment, for the total mortality of such wounds in war, where operative interference is rarely adopted, has been so far 50 per cent.

THE SURGERY OF NON-MALIGNANT DISEASE OF THE STOMACH.

W. H. BROWN, M.R.C.S. ENG. COLAC, VICTORIA.

Non-malignant stomach surgery seems to me to fall naturally into two divisions according to the two aims towards which surgical intervention may be directed. Briefly stated, the two aims are these:—

We may operate to bring about the cure of breach of continuity of one of more coats of the stomach wall (this would include operations

† *La Semaine Médicale*, 3 Juin, 1908, page 275.

‡ Walton Martin. The lesions associated with Gunshot Wounds of the Stomach. *Annals of Surgery*, May, 1907, page 699.

for ulcers or for wounds of the stomach; or we may operate to restore the impaired function of the stomach, looked at as a part of a contractile tube, whose business it is to pass on the substances which come into its cavity. Doubtless both these aims have to be before us in many, indeed in most cases of operation; doubtless also the methods which we use towards one aim may be of use also in pursuing the other, but the two aims *per se* are distinct.

To speak first of the surgical treatment of gastric ulcers. I would first express my conviction (perhaps a trite one) that any case that gives fair grounds for the suspicion of a gastric ulcer ought, after having been submitted to one reasonable and thorough (but unsuccessful) attempt at cure by dietetic and pharmaceutical methods, to be operated on. What are fair grounds for the suspicion of a gastric ulcer? The phrase must certainly not be held to include (in my opinion) all the classical symptoms, such as hæmatemesis or even vomiting; that persistent pain may be the only symptom for years, the following case will show:—

Some six years ago, a young man, a jockey, aged 25 years, came to me complaining of pain, which he described as being deeply situate below or beneath the left lower ribs. He said he had suffered with it for four years, and had been under a number of doctors, but the pain got no better, so he came to me with the express desire that he should be "opened." I gave him some medicine and sent him away for three weeks. At the end of that time he returned no better. I took him into the hospital for observation, and kept him for some weeks. He never vomited, had never done so through the course of his illness; neither a milk diet, nor total abstinence from all but water by the mouth had any influence on his pain, which seemed to be always present. His temperature, urine, leucocyte count were all normal, nor could anything whatever be detected by palpation or percussion; there seemed to be the persistent aching pain, and nothing else. At length with some misgiving, but at his urgent entreaties, I operated, opening the abdomen through the left rectus as high up as possible. After some fruitless search I felt what seemed to be a thickening like a half-penny let into the wall of the stomach in its lesser curvature, and near the cardiac end. I could not bring it out of the wound, so got at it by opening the stomach; then I saw the typical round, deep hard-edged callous ulcer, no larger than a threepenny bit in itself. I did the only thing that seemed possible—excised it from the inside, cutting out all that seemed unhealthy tissue. I then (at that time) closed up the gap by super imposed layers of continuous suture with very fine silk, finishing with the mucous membrane. The man did well, was at once relieved of his pain, and six months later remained quite well.

With regard to operations in general for gastric ulcer (not perforated). I consider the ideal method undoubtedly to be the complete excision of the affected area of the stomach wall, including all the coats, whether or not one adopts Moynihan's advice and performs gastro jejunostomy as well; but the conditions which make such an ideal method feasible seem, from my experience, to be somewhat uncommon. Either the size of the affected area, or its situation, or its firm adhesion to surrounding parts (especially the liver), frequently make its excision doubtful or impossible. What prospect in such cases does gastro enterostomy, viewed as the alternative method of operative treatment, hold out of cure? I should say that it gives a fair, but by no means a certain prospect of cure.

I have operated on several cases of the following kind:—There have been symptoms long persistent, such as to give good grounds for a clinical

diagnosis of gastric ulcer, and such symptoms have not been permanently relieved by a thorough course of medical treatment, including rest in bed, rectal feeding for a time, followed by strict dieting; but on opening the abdomen, and indeed also the stomach, I have not been able to find any ulcer. Here I have done a gastro enterostomy, with the result of a lasting cure of the symptoms. I am aware that this is against current teaching, and I am not recommending the practice, but merely recording the fact.

In other cases (two) with a similar history, and after a similar ineffectual course of medical treatment, I have found on opening the stomach a more or less complete ring ulcer, situated quite close to the pyloric opening. To excise such an ulcer would need a pylorotomy. In place of that, in both these cases, I did a gastro jejunostomy with completely satisfactory results.

Another somewhat similar case is perhaps sufficiently instructive to bear recording in detail.

Five years ago, a man aged 40, otherwise healthy, was under my care for many months suffering from symptoms referable to an ulcer of the stomach. There was occasional vomiting, but pain was the principal symptom—pain which was said to be always worse about five o'clock in the afternoon, and to be favorably affected only by the ingestion of "ginger nuts." I have noticed in this case, as in others like it, a peculiar facial expression suggestive of, and caused by the more or less continuous pain. At length operation was consented to; I found much thickening, suggesting new growth near the pyloric end of the stomach. On opening it a large deeply hollowed ulcer was seen. I decided on the whole that the ulcer might probably be non-malignant, and resolved to try a gastro enterostomy, rather than the extensive resection, which would otherwise have been needful. I did a posterior retro-colic anastomosis, using a medium-sized Murphy button.

The immediate result was quite satisfactory, the man quite losing his pain and other symptoms. The button did not come away. This relief lasted for ten weeks after the operation, then the old symptoms began to return, and before long were nearly as bad as ever. Just five months after the first operation I re-opened the abdomen and the stomach. The ulcer viewed from the inside was markedly smaller than when seen before, but some external appearances inclined me now to the view that the disease was malignant. The Murphy button was still in situ, with its canal open, and its edge gripping (but not forcibly enough) the stomach wall, transverse mesocolon and jejunal wall.

I decided to resect, and did so, cutting through the duodenum on the one hand, and the stomach in Mikulicz's line on the other. About two-thirds of the stomach was thus removed, the old anastomosis opening being closed (after removal of the button) by turning in stomach wall.

The man did well, and has remained quite well ever since (four years). The specimen removed was sent for examination and reported non-malignant.

The above cases give clear evidence, I think, that gastro enterostomy alone has a favorable influence on gastric ulcer, and may reasonably be adopted with considerable hope of cure, where circumstances contra indicate the resection of the ulcerated area.

On the other hand, I operated a year ago on a man also aged about 40, with symptoms very similar to the last one. I found the posterior wall of the stomach very firmly adherent to the under surface of the liver, and on opening the stomach a chronic ulcer as large as a florin

was seen. There was nothing to point to malignancy. I did an anterior gastro enterostomy as low on the anterior surface of the stomach as possible, using a button here too, as stitching would have been difficult. The patient did well, and the button was passed on the 19th day, but his symptoms have been practically unrelieved, pain, flatulence, and occasional vomiting, existing much as before, though he has not lost weight or suffered in his general health.

I come now to record briefly my experience of that other department of gastric surgery, which deals with obstruction, the inability of the stomach to pass on its contents at the normal rate, and in the normal way. Such inability may arise from various causes; most commonly from cicatricial contraction (from an old ulcer of the stomach wall) at or near the pylorus; also from involvement of the pylorus in adhesions externally, due to gallstone, or other disease; from spasm of the sphincter (without organic contraction), due to an ulcer near the pylorus. (This, I think, must explain those curious cases where, in addition to persistent symptoms of a gastric ulcer, there are intermittent attacks of obvious obstruction with dilatation of the stomach.)

I have one such case, where the patient, a healthy young man, has always a foul tongue, and is never quite free from some gastric pain, or discomfort; yet, as a rule, he goes along fairly well, and does not feel ill enough for serious treatment. He gets, however, occasional attacks of severe pain with vomiting; these attacks present all the features of pyloric obstruction and gastric dilatation, and during one of them, on passing the stomach tube, I removed 84 ounces of fluid from the stomach.

Another, but rare cause of inability of the stomach to pass on its contents is cirrhosis of the stomach wall. And lastly, we come to the cases known as acute dilatation of the stomach.

The accepted surgical remedy for the above conditions (leaving the last-mentioned aside for the moment), is a short-circuiting anastomosis, gastro enterostomy in some form or other. I think one may take it that pyloroplasty does not now come into serious competition with gastro enterostomy. My own experience of the latter operation in cases of non-malignant pyloric obstruction would lead me to consider it entirely above criticism, the results, both immediate and remote, seem to be amongst the most satisfactory that surgery can ever achieve. That point must now be a matter of common experience; to narrate cases to prove it seems wholly unnecessary.

I wish therefore only to speak of the value of gastro enterostomy in the class of cases known as acute dilatation of the stomach. This affection was the subject of a most exhaustive essay by Laffer in the March and April numbers of the *Annals of Surgery*. His conclusions are that while its cause is not certainly known, the evidence certainly points towards disturbance of innervation as a frequent, if not constant cause. He makes the mortality to be 63.5 per cent., and considers the cardinal symptoms to be: distention, copious vomiting, no rise of temperature, rapid pulse, great thirst, little tenderness, and increasing collapse. The following case showed all the above, with the exception of the second (vomiting) which was absent:—

A young woman came to me four and a half years ago complaining of having suffered from "indigestion" for several years, during which time she had been under several doctors. She said that pain in the right half of the epigastrium had been her principal trouble. During one week's observation in hospital, sickness was a marked feature, the vomiting

occurred immediately after taking food, not always, but mostly. The food was expelled unaltered. There were also attacks of pain in the right epigastrium (three or four during the week) These were very severe, but they had no relation to the sickness, which they never accompanied. There was always marked tenderness in the right epigastrium.

I thought we had to do probably with an ulcer in or near the pylorus, with surrounding adhesions. However, on opening the abdomen I found an irregularly-shaped hydatid about as big as a turkey egg. It lay beneath the gall bladder, to the under surface of which it was firmly adherent, as also to the anterior surface of the second part of the duodenum.

The entire tumour was removed by dissection, and the area drained through a posterior puncture. The case did perfectly well for the first four days, with the exception of a little vomiting; on the fifth day, however, the patient began to appear strangely prostrate. The vomiting had ceased, the temperature was below 99F., the bowels acted, there was moderate abdominal distention, but no marked tenderness, the pulse was about 100 weak, and small, the tongue inclined to dryness, and there was thirst, but by far the most remarkable feature was the intense prostration. The patient lay on her back with features relaxed, she felt she was going to die, and she looked like it.

I confess I was puzzled as to the cause of the condition. It was not till the evening of the seventh day after operation that I suspected dilatation of the stomach. I at once endeavoured to pass the stomach tube, but could not succeed in inducing the girl to let me pass it. She implored only to be allowed to die in peace. In desperation I gave her chloroform, and then passed the tube; it evacuated four pints of greenish fluid from the stomach. The girl felt better after this, but the old condition was recurring next day, while still nothing would induce her to allow an attempt to pass the stomach tube.

She was exceedingly weak, and her pulse was now 120. I decided to try gastro-enterostomy as a last resource. I obtained permission from her relatives to make a last attempt to save her, and under light anæsthesia, I re-opened the abdomen and, working as quickly as I could, while a saline infusion was being poured into the submammary space, I did a posterior retro colic gastro-enterostomy, using a small Murphy button for the anastomosis.

The result was all we could have hoped, the patient rallied, and thereafter gave us no further anxiety. The button was passed on the seventh day. The patient soon became well and strong. I feel certain she would have died but for the gastro-enterostomy.

Laffer, in the paper above referred to, mentions that gastro-enterostomy has been recommended for this grave affection. Doubtless it has been done in similar circumstances, but I have not come across any reports of cases.

It only remains to mention briefly the conclusions one has been led to on the subject of technique. I consider that the operation of election in all cases, where a gastro-enterostomy is desirable, is the posterior, retro colic, so-called "no loop" operation, the posterior wall of the right half of the stomach being made to protrude through a tear in the transverse mesocolon and anastomosed with the anti-mesenteric border of the first part of the jejunum.

I use large clamp forceps for stomach and bowel, and stitch them together with an outer continuous suture of linen thread, and an inner one

of No. 1 catgut. At the same time my experience has given me good confidence in Murphy's button for this operation, if the need for quickness or other circumstance should indicate its employment. I have done pyloroplasty and Finney's operation gastro-duodenostomy, but they seem to me to present no advantages over gastro-jejunostomy, and my feeling is in favour of the latter procedure.

THE SURGERY OF NON-MALIGNANT DISEASES OF THE STOMACH.

JOHN RAMSAY, M.B., M.S.; LAUNCESTON, TASMANIA.

With a subject of such vast importance, and about which a wealth of medical literature is so rapidly accumulating, one feels that from a comparatively limited practical experience it is impossible to do more than touch upon a few of the points that seem to have forced themselves under one's notice, as worthy of submitting for your consideration.

Perhaps the fight has waxed most fiercely around the question of gastro-enterostomy and its technique. A few years ago one would have thought that the anterior was doomed as the routine method, and only to be availed of when the posterior junction could not be effected owing to such difficulties as extensive posterior adhesions or a short mesocolon. But a new life has been given it by the support of Paterson, Spencer, and others, so that again we are compelled to decide for ourselves the troublous question when called upon to operate.

That the anterior operation is simpler in execution and associated with less disturbance of viscera is apparent, and the possibility of performing it under local anæsthesia further renders it tempting; that the opening made soon drags into a suitably dependent position has been shown by *post-mortem* examination; and that the physiological effects and remote results have been satisfactory, weighs also in its favour. But the long afferent jejunal loop theoretically appears to be unsound, not to say dangerous, from possibilities of volvulus, internal hernia, and colonic obstruction, with a greater chance also of regurgitant vomiting. Improvements in technique have undoubtedly rendered complications of this kind less probable. Amongst others, I would emphasize the necessity of a sufficiently long loop (12-15 inches from the duodeno-jejunal flexure), avoidance of excessive rotation of the loop, an adequate opening, and the use of a few gastro-jejunal "anchor" sutures on either side of the gastro-enterostomy opening. The posterior method of von Hacker appears to be the more theoretically correct, were it not for the opening required in the mesocolon, which is unavoidable. The natural position of the viscera is less disturbed as a result, the opening is dependent in both erect and recumbent positions, and provided that certain details are attended to, complications are less likely to arise. The direction of the opening, whether obliquely downwards and to the right, obliquely downwards and to the left (Mayo), or vertical (as adopted more recently by Moynihan), does not seem to be a matter of much importance, provided that it extends almost to the greater curvature, and more important still, that it is of sufficient size—a minimum

length of $2\frac{1}{2}$ inches being desirable as insisted upon by Barker. Especially should this be so in greatly dilated stomachs, for diminution in the size of the opening often occurs concurrently with the subsequent shrinkage in the size of the organ. The edges of the mesocolic opening should be fixed by a few sutures to the adjacent jejunum in order to prevent hernia through the opening. Mechanical appliances of the nature of buttons or bobbins to secure union of the surfaces are undesirable, as the best results are obtained by suture, with the aid of clamps. Of various patterns I prefer Moynihan's. For suturing a continuous through and through one of chromicized gut and a sero-serous one of Pagenstecher thread are employed, an ellipse of mucous membrane being excised from both viscera to insure patency. An experience with Murphy's button in one of my earlier cases convinced me of its ineffectiveness in providing a satisfactory permanent opening. A second operation was required in a case of pyloric stenosis owing to the recurrence of symptoms about nine months later, and on investigation the opening could hardly be seen, and only admitted an ordinary probe. To get over this difficulty, which was assumed to be partly due to the still patent but constricted pyloric opening, Eiselberg's suggestion to obstruct the pylorus by a ligature was adopted in a subsequent case, but the Murphy button was dispensed with, and the precaution taken of making an adequate opening by suturing. The experience of Cannon and Blake with dogs, using X-rays after internal administration of bismuth, showed that with one exception in which a very large gastro-enterostomy opening was made, the gastric contents usually continued to pass through the pyloric channel after operation. It might be argued that prevention of this by pyloric ligature might compel the maintenance of the artificial opening. Further experience has shown this to be unnecessary. The beautifully elaborated technique adopted by Moynihan has since proved highly satisfactory.

In what part of the stomach—cardiac or pyloric—shall our gastro-enterostomy opening be made.

The majority of surgeons consider that the pyloric portion is the site of election, whilst others, like Mansell Moullin, consider the cardiac that of choice. Investigations by Gray with the fluorescent screen after bismuth show that where gastric motility is good, the cardiac portion is concerned for some considerable time in the valuable mechanical process of churning, before the contents are passed onwards to the pyloric part, a contracted portion—a middle sphincter—being evident in most stomachs between cardiac and pyloric portions. Whereas in cases where the motility is impaired, the food soon passed through the cardiac part and out by the stoma with the patient in the erect posture. Kocher and Kelling also state that in the pyloric portion some muscular closure of the opening is obtained. It would seem wise, therefore, to avail ourselves of as much physiological action as the organ is capable of, and to make the opening in the pyloric portion. The presence of scarring or adhesions in this part might lead us, however, to choose the proximal part, and where dilatation is great and motility impaired, I think that the site selected should be that which is considered to be the most dependent when the patient is in the upright position.

Gastro-enterostomy is, however, not the whole of gastric surgery in non-malignant disease, though by far the greater part. My earlier experiences were those of pyloroplasty for pyloric stenosis, and fortunately the first two proved satisfactory, as the thickening was not great, and the

ulcers had long since healed. Morison had four relapses in 27 cases of pyloroplasty, and Mayo 7 in 21. I have seen Finney perform his gastro-pyloro-duodenostomy, and followed his example in one case, which unfortunately died about a fortnight later for no special cause connected with the operation, but, apart from its unsuitability for cases with extensive pyloric adhesions or great induration, the mortality, according to the latest statistics, reaches 6 to 9 per cent., whilst the results of the simpler gastro-enterostomy have proved equally good, though theoretically his operation may seem to be more correct. The same remarks apply to the various methods of gastro-duodenostomy, which do not appear to permit of satisfactory emptying of the stomach, especially when the organ is greatly dilated or sacculated.

The older operation of pylorectomy when it is considered advisable to excise tumour-like masses of inflammatory tissue or dense scars, is likely to be replaced by that of Rodman, who adds to a gastro-enterostomy an excision of the diseased pylorus with closure of the gastric and duodenal ends.

The well-known post-operative complications of gastro-enterostomy have to be guarded against or contended with. With Terrier, I am disposed to think that certain technical errors are responsible for their frequency. Regurgitant vomiting has, in the experience of Moynihan, Paterson, Robson, and others, become rarer with improvement in technique. The no-loop method, a free opening, an extensively fixed jejunum are important factors in its prevention, whilst careful after-treatment, the sitting posture, lavage on the early appearance of vomiting, and a restricted dietary, even to abstinence and the substitution of rectal alimentation seem to render serious cases of this nature less frequent. The condition is evidently an obstructive one, probably of a mechanical kind. Entero-anastomosis usually does relieve, but in serious cases has not always done so. The Y-anastomosis of Roux has proved satisfactory in preventing it, but more recent experiences show that this more extensive operation is hardly necessary as a primary, but may be of value as a secondary operation in long-loop cases. Peptic ulcer of the jejunum is a much less common complication. Generally ascribed to the hyperacidity of the gastric juice acting on a jejunum unaccustomed to it unneutralized, the remedy should lie in the reduction of this condition. The investigations of Paterson show that after gastro-enterostomy an average fall of acidity to the extent of about 35 per cent. takes place, and Willcox has found the total acidity to be diminished in five cases. One interesting case (quoted by Paterson) showed that where this fall did not take place a contracted gastro-enterostomy opening was present. A free opening may therefore lead to its prevention, and where post-operative examination of gastric contents show a maintained hyperacidity, it would be wise to neutralize for some time with doses of an alkali. I have no practical experience of this condition.

In all gastric operation cases, where possible, some preparatory treatment should be carried out before operation. That the contents of 50 per cent. of healthy stomachs are sterile has been shown by Hewitson. Cushing has shown that by the administration of sterile food a satisfactory condition of the stomach contents may be obtained. The carious teeth require attention, so as to prevent the ingestion of pathogenic flora from the mouth. The use of antiseptic mouth washes with a toothbrush every few hours for a few days is desirable, and, if possible, at hourly

intervals immediately preceding operation. For twenty-four hours preceding operation only sterile water, weak tea without milk, and liquid peptone are given by mouth, supplemented by nutritive enemata with saline. Lavage is, I consider, unnecessary, unless there is marked gastric stasis with fermentation, especially of an offensive character. By these measures some effort is made to attain asepsis. By the subsequent use at operation of gauze pads and strips suitably placed, and the resterilization of instruments used for opening the viscera, this condition is also promoted.

In what non-malignant diseases then is gastro-enterostomy advisable? It may be required, in the first place, as the operation of choice, and, secondly, as an auxiliary operation. With gastric ulcer and its complications the operation plays its greatest part. A typical case is the ordinary pyloric stenosis with dilatation and gastric stasis. Gradual failure of general strength in spite of medical treatment, not overlooking the hygiene of the mouth, is a powerful indication for its performance. Where a condition of ill-health is maintained with recurrent or continued distressing symptoms, vomiting, discomfort, the operation is recommended as giving an excellent chance of complete relief from symptoms, as well as a restoration to good general health. The increase of weight following such operations is in many cases surprising. The operation is preferable to repeated lavage with most patients.

Patients with recurring symptoms of gastric ulcer should also be advised to submit to operation, provided that they have had the opportunity for thorough medical treatment. That treatment should not be ambulatory. A month or more in bed, rest to the stomach at first, except perhaps for the administration of bismuth and soda, attention to oral sepsis, rectal alimentation, perhaps extended over a fortnight; this to be followed by a milk diet for a fortnight or more. Patients, as a rule, tolerate the rectal injections well; peptonized milk, sugar, liquid peptone, being given with normal saline solution every four hours or so. Latterly, continuous saline by the drop method has been satisfactorily adopted for a few hours daily for the relief of thirst. In the severe cases, no solid farinaceous food is given for three or four weeks, and after that the diet is regulated for the next few months. In our experience at the Launceston Hospital, it is not common to have cases of relapse. During the past ten years, out of 120 cases of probable ulcer, only eight have returned to hospital. Unfortunately, I have not been able to follow up the cases more closely. This experience, even allowing for doubtful diagnoses or removals, is so much at variance with that of others in the Old World (*e.g.*, Bulstrode found 40 per cent. of relapses in the apparently-cured London Hospital patients), that I shall be glad to hear from members whether or not, under Australasian skies, their experience has been of a like kind. All cases presenting themselves in the out-patient department with symptoms suggestive of gastric ulcer are advised to come into hospital, and the advice is insisted upon if medical treatment for a week or two does not relieve. The mild or doubtful cases are treated less strictly.

In the absence of pyloric obstruction, I am not disposed to submit to operation cases of chronic ulcer with symptoms extending over a period of months or years until a thorough trial has been given of treatment in bed. Should relapse occur after apparent cure, operation would be advised. The possibility of cancer developing upon an ulcerous base has become more and more evident in recent years, until from Rodman's

statistics we find that in the experience of five surgeons (Mayo, Moynihan, Robson, and himself) cases of cancer of the stomach were found to have a history of ulcer in from 54 to 90 per cent., and in all of Rodman's last nine cases. Haaser has estimated that 6 per cent. of ulcer cases are followed by cancer. Such statistics as this should lead to less hesitation in considering the question of operation for gastric ulcer, and should bring more frequently into favour the operation of excision of the ulcer base in cases, where up till recently, gastro-enterostomy was the usual operation.

The presence of induration of inflammatory or undetermined character at the pylorus, either in connexion with ulcer and perigastritis, or perhaps the result of chronic pyloric spasm in hyperacidity, would render gastro-enterostomy advisable, and, in addition, the scarred tissue could be excised as recommended by Mayo, or a specimen removed for purposes of microscopic examination.

Akin to these inflammatory masses are the numerous examples of perigastric or extraduodenal adhesions, resulting either from the peritonitis of deep ulcer or, in my experience, more commonly from gall stones and their complications. These may lead to kinking or other modes of obstruction at or near the pylorus, causing such gastric stasis that gastro-enterostomy is required for its relief. An old man of 74 years, on whom I had operated about six years previously for suppurating hydatid on the under surface of the liver, returned in an emaciated condition with signs of pyloric obstruction. He died before there was time to prepare him for operation, and dense adhesions were found at the side of the hydatid, with enormous dilatation of the stomach. In many of these perigastric-adhesion cases it may be sufficient to divide them if band or wire-like, or to free them thoroughly, at the same time covering the raw area with some substance likely to prevent reformation of the adhesions. In these cases I have had no experience of the Cargile membrane, gold-beaters' skin, or the Vögel method of covering with a gum-acacia solution, as suggested by Robson; but I have frequently employed an omental graft or flap for this purpose with evidently satisfactory results. The employment of gastro-enterostomy would be advisable if the adhesions were extensive and the lumen of the gut narrowed.

Hour-glass stomach, usually the result of cicatrization of gastric ulcer with or without perigastric adhesions, gives the surgeon ample opportunity for choice between the different methods of promoting exit from the various cavities. Excision of the diseased area—partial gastrectomy—where possible, is recommended by Mayo, and seems the ideal operation; but there is plenty of variety at one's disposal in gastro-enterostomy, single or double, gastropasty, gastro-gastrectomy, or combinations thereof.

Our knowledge is perhaps most limited in regard to the treatment of hæmorrhage into the stomach. I confess that I have been frequently tempted in cases of severe hæmorrhage to adopt one of the elementary principles of surgery and secure the bleeding point, but, so far, have refrained. In the severe cases of acute gastrorrhagia, one is reluctant to submit the patient to almost inevitably fatal operation, for even in the most hopeless, general treatment has frequently proved satisfactory. Besides morphia, adrenalin by the mouth for its local effect (it is said to produce no constitutional effect when in the stomach), and calcium chloride, given in the saline-rectal injections, are usually employed, and, so far, there has been no death from hæmorrhage with us. The experience

of different surgeons and physicians varies greatly on that point. At Guy's, out of 7,500 necropsies, only four were found to have died of hæmorrhage. I would feel inclined to apply the principle of operation in the same way as in recurrent gastric ulcer. Continued or recurrent gastrorrhagia not subsiding under proper treatment, and provided that the patient is considered to have a fighting chance, may be treated by exploratory operation with a view to discovering and treating, if possible, the cause of the hæmorrhage. How frequently any satisfaction is gained, the records of medical literature tell! Hale White collected 29 cases of gastrostaxis, in many of which surgical measures were employed and no ulcer found. Though successful gastro-enterostomies are reported by Dawson, Robson, and Moynihan, there were others in which no relief was afforded, and others, again, where the exploratory gastrotomy was followed by relief. Ligature of bleeding points may be of service if they can be discovered. In ulcer cases, gastro-enterostomy alone may effect a cure without dealing with the bleeding point, but in suitable cases it would seem wise to combine with it excision of the ulcer. Mikulicz advises jejunostomy as an additional measure.

Perforation is a surgical emergency that any medical practitioner may have to deal with. Fortunately, the anterior surface of the stomach is that usually affected. Its symptoms are not infrequently simulated by other acute abdominal conditions in the subjects of chronic gastric ulcer. It seems a far cry from ruptured tubal pregnancy to perforated gastric ulcer, but I must confess that in a case with gastric symptoms and metrorrhagia, after much deliberation, I have opened in the epigastric instead of the hypogastric region. There is obviously only one treatment for perforation, viz., suture, preferably in two layers, one for the edge of the ulcer, the other an inverting one. Excision, as a rule, is unnecessary. In addition, where constriction of the stomach lumen is produced by the suturing, especially near the pylorus, a gastro-enterostomy should be performed if the condition of the patient permits of it. If a localized peritonitic collection is present, it should be drained; if general, a further incision in the suprapubic region is advisable, with drainage of the rectovesical, or the pouch of Douglas. Others may be required in the loins. Wiping with gauze will remove most of the *débris*, but where the exudate is copious, free saline irrigation may be required. In some cases in the early stage, the epigastric incision has been closed without drainage; but I have never tried this. Fortunately, the peritoneal contents in such cases are frequently found to be sterile (Caird, four in eleven cases; Dudgeon and Sargent, three in nine); the special gram-negative strepto-diplococci of the two last-named investigators seemingly confining themselves to the walls of the ulcer itself, although at times found in the exudate. I believe in the value of the Fowler position and continuous saline by bowel.

It is not often that hydatids burst into the stomach, but two cases have occurred in our experience at the hospital. In one, the rupture occurred at operation—a source of alarm to the anæsthetist owing to the vomiting of fluid. It healed without being sutured or the formation of a gastric fratura. In the other septic case, where suture was employed, the weak old man died a few days later.

The position of operation in relation to the hypertrophic pyloric stenosis of infants is at present not unanimously determined, owing to the high mortality of the operation and the success that has frequently followed lavage and proper dietary. No doubt, with failure of medical treatment,

surgical measures may be required, as the condition will otherwise prove fatal. I have only seen cases in the last stages too weak even for the consideration of surgical measures.

In gastropsis, pure and simple, gastro-enterostomy can hardly have a place, and whilst averse to any method of visceroparietal fixation, I think that in Beyer's operation, directed to the shortening of the long gastro-hepatic omentum and gastro-phrenic ligament, we have a surgical measure of great utility at our disposal.

In Brewer's experience, gastro-enterostomy has failed in relieving gastric crises of tabes, but general experience shows it to be the operation *par excellence* for the tetany of gastric origin.

In this paper I shall not attempt to touch upon many of the rarer diseases of the stomach; but, in conclusion, will deal briefly with the large mass of clinical material in the various forms of chronic dyspepsia which constitutes such a large bulk of general practice. When all medical measures have been exhausted, the surgeon is pathetically implored to render service. It was naturally hoped that with such a reputed "drainage" operation as gastro-jejunostomy, the journey of the ingesta would be hastened, and no time afforded for the operations of fermentative bacteria. We know, from Morton's observations in the subjects of gastro-enterostomy, bismuth has reached the cæcum in the short space of one and a half hours instead of the regulation four to five hours, and the early passage of the bismuth through the stoma has been seen on the fluorescent screen. I am looking forward with hope that some light may be thrown on this ill-illuminated subject. I have met with no case of hyperacidity, in the absence of ulcer, where surgical treatment was considered, and am unable to find much evidence in literature of relief of such a condition, though the operation is advised for it. Improvement would be expected from the investigations of Paterson, Willcox, and others, who find that the total acidity is reduced after gastro-enterostomy.

For the large class of atonic dyspeptics with impaired motility, the results of operation have at times been satisfactory, *e.g.*, Deaver reports six cases of gastrectasis with four cures and two deaths; whilst, with others, the results have been negative. The exploratory operation is, without doubt, desirable in many of these sufferers from chronic dyspepsia unrelieved by medical treatment, chiefly in the hope that some indication of old ulceration may be found and satisfactorily dealt with. In the large class of neurotics and neurasthenics especially, frequent performance of gastro-enterostomy would be likely to bring the operation into discredit. I have tried to operate on two of these cases, but in one the patient took the anæsthetic so badly that exploration only was performed, whilst in the other a gastro-enterostomy by Roux's method was done under local anæsthesia. In spite of the protestations of the latter patient to the contrary, his condition appeared to be no better. Gastro-plication has been tried and found wanting, and perhaps the best that can be said of it is that it may give a better opportunity for satisfactory medical treatment.

Moynihan, in his address to the American Surgical Association of this year, emphasises this by saying that—"The operative treatment of stomach disorders should be confined exclusively to those cases in which an organic lesion is present." Finally, one might add that the treatment in most cases of stomach disease does not end with the performance of the operation, and that much of the success in operative results must be attributed to a careful after-treatment.

DISCUSSION ON DISEASES OF THE STOMACH.

MR. F. D. BIRD (Melbourne) had always performed gastro-enterostomy by the no-loop method—as to the position of the opening, he simply chose the most comfortable and easy one. Regurgitant vomiting was generally checked by sitting the patient up. He was doubtful as to the advisability of gastro-enterostomy when the stomach was very greatly dilated. He did not suture the meso-colon. It was better not to perform gastro-enterostomy in perforated gastric ulcer, but he would do it in duodenal ulcer.

MR. H. C. HINDER (Sydney) had done 33 operations, with two deaths; one was from faulty technique, using Murphy's button. He always performed the anterior operation. He started with it and found it satisfactory. The stomach was not like a tinpot, but adapted itself. He always used a short loop. He had two cases of regurgitant vomiting. In one case sitting up gave no relief, though the patient recovered. In the other, he re-opened on the fifth day, and did gastro-enterostomy—the patient recovered. He thought the case of acute dilatation mentioned by Dr. Brown might probably have recovered without operation. As regards perforated gastric ulcer, he had done gastro-enterostomy in two cases, and both recovered.

DR. STEER BOWKER (Sydney) had always performed anterior gastro-enterostomy, and had never had regurgitant vomiting. Formerly, he used Murphy's button, now he sutured, but had given up Pagenstecher's thread, as it was difficult to pull through. He was doubtful as to performing gastro-enterostomy in ulcer of the stomach, as peptic ulcer may form afterwards.

DR. H. L. MAITLAND (Sydney) had formerly performed anterior gastro-enterostomy, lately he did posterior—each gave equally good results. Excision of gastric ulcer was good theoretically, but disappointing in practice, owing to the continuance of acidity—gastro-enterostomy was sufficient. As to hæmorrhage from acute ulcer, he had always waited, and the patients died; in future, he would do a gastro-enterostomy after the shock diminished.

DR. E. S. JACKSON (Brisbane) had given up Murphy's button, and always sutured in gastro-enterostomy. As to diagnosis, he found that if a patient's symptoms were not relieved by rest and simple measures, the case was one of neurasthenia, and the more pitiful the complaint the more probably were they neurotic. He had never seen a death from hæmorrhage in acute ulcer of the stomach.

DR. W. MOORE (Melbourne) had followed the practice of Dr. Mayo. He used to use the Murphy button with good results; but now sutured. In experienced hands there was little difference in the time taken, but in less experienced the button was quicker and safer. He thought the meso-colon should be sutured, and narrated cases where obstruction had occurred from intestine passing through the opening. As to acute hæmorrhage, patients generally recovered without operation.

MR. G. A. SYME referred to "hour-glass" stomach. In one case he performed gastro-enterostomy, in another gastro-gastrotomy, and both did equally well. In regard to acute hæmorrhage, he had never operated

during the attack, but he had seen one case that was fatal. In chronic ulceration, gastro-enterostomy was not always sufficient; he had seen hæmorrhage and other symptoms recur afterwards; and when the thickening was very great, a partial gastrotomy was indicated, which he had done in one case successfully.

DR. FIASCHI (Sydney), in reply, said that acute dilatation was best dealt with by postural treatment, and was often due to strangulation of the duodenum by omentum.

INTRODUCTION TO THE DISCUSSION ON THE SURGERY OF PARALYSES.

BASIL KILVINGTON, M.D., M.S., MELBOURNE.

I have found some difficulty in introducing this discussion, as it is impossible to deal with all the items of such an extensive subject.

I have limited my remarks to the surgical treatment of facial paralysis, and anterior poliomyelitis, because the advisability of operative interference in suitable cases of these conditions is admitted. At the same time, the experience of any individual is too limited, and the subject too recent, to dogmatize on the exact methods used.

In the case of facial paralysis, the only surgical relief is that given by nerve anastomosis.

A good many cases have been reported, and the results have been so good that it should be more widely practised.

It is universally admitted that the longer the paralysis has existed the less is the chance of subsequent recovery, consequently the operation should be performed as soon as we can decide that return of power will not occur under massage treatment. It is only exceptionally that nerve anastomosis can be justified (immediately). Such cases are those of undoubted division of the facial in mastoid operations, or in removal of parotid tumors.

In cases of persistent facial spasm, where the position has been explained to the patient, and he consents to the risk, we are dealing with undegenerated nerve fibres, and the results should be excellent with the immediate suture. The vast majority of facial paralyses result from non-traumatic causes, *i.e.*, Bell's paralysis, or else from mastoid operations, where it is uncertain if the nerve has been actually divided.

The rule usually followed is to wait six months, and if there is no improvement, to advise operation. In estimating improvement, we should rely mainly on return of voluntary power. In many old cases, a condition of contraction, resulting in deepening of the nasolabial fold, and drawing of the angle of the mouth, gives a false impression of commencing recovery.

Personally, I am inclined strongly to favour electrical prognosis, which allows of much earlier operation in suitable cases. Electrical reactions are of no value within ten to fourteen days of the onset, because it takes this time for the degenerative processes to go down the nerve.

According to Purves Stewart, if typical R.D. is obtained, recovery will not commence for at least three months, and will probably be imperfect and associated with some contraction. If partial R.D. be found, recovery may be expected in six to eight months. If the contractions are only diminished and not abolished to Faradism, recovery may be looked for in from three to six weeks. Probably all the first group should have the advisability of nerve anastomosis placed before them.

There seems no absolute time limit in the other direction, for Elsberg has reported a recovery in a case of 29½ years' duration. It is important to get members' experience on the question of the suitable time for interference. Three nerves have been recommended for reinforcing the facial, the spinal-accessory, the hypoglossal, and the glosso pharyngeal. The last was suggested by Professor Schafer, as its motor nucleus is near and homologous to that of the facial.

I am not aware that it has ever actually been used, and the operation would be most difficult. The choice then lies between the spinal accessory and hypoglossal, and most surgeons prefer the latter.

The main disadvantages to be considered are the associated movements of the tongue and shoulder with the face, the more or less weakness of the muscles supplied by the reinforcing nerve, and the readjustment of co-ordination necessary.

The associated movement of the face and shoulder are very conspicuous and distressing, and occurred in all of Ballance's cases, but after some years it is stated to have disappeared in some. Where the hypoglossal is used, the face works during articulation and swallowing.

These disadvantages only hold where an end to side anastomosis is used. When the reinforcing nerve is completely divided and sutured to the distal end of the paralysed trunk, it does not occur, but there is of course hemiatrophy of the tongue or dropping of the shoulder, with wasting of the sternomastoid and trapezius.

With regard to cortical control, the facial and tongue centres are closely connected, and work to a certain extent together. From this point of view, the hypoglossal is preferable.

On the contrary, where the operation is done for facial tic, this would be a disadvantage. I have a case of obstinate facial spasm of 25 years' duration, in which, for this reason, I propose to use the spinal accessory.

METHOD OF NERVE ANASTOMOSIS.

Nerve fibres of the reinforcing nerve have to be divided, to be given an opportunity of growing down the facial, so that any procedure which only opens the nerve sheath is to be condemned.

For the same reason, a longitudinal slit in the nerve is uncertain, and divides an unknown number of fibres at different levels.

The three procedures to be considered are:—(1) Making a transverse cut, one-third to half way across the reinforcing nerve, and implanting the divided facial in the gap. (2) Splitting a flap off, or, if possible, turning up a large faciculus from the reinforcing nerve and joining it to the distal end of the facial. (3) Using the whole of the reinforcing trunk.

(1) When a gap is made in the healthy nerve and the facial implanted there, the cut fibres in many cases divide, sending one branch down a

facial track and another to tongue and shoulder muscles. In this way a single motor cell is innervating two sets of muscles, and associated movements are inevitable. (Fig. I.)

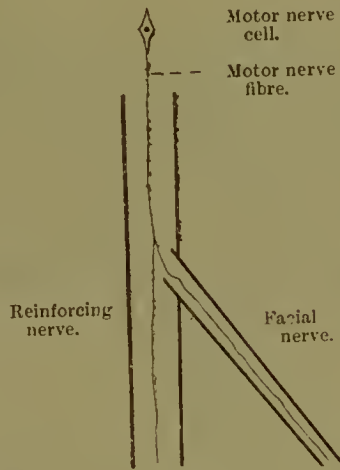


FIG. I.

Possibly, after some years, this may be overcome by the patient learning not to employ these fibres. It is more likely that he uses adjoining ones to antagonize these and render their action unnoticeable. In any case, this entails a great waste, and this method, which is at present recommended in the text-books, should, in my opinion, be abandoned.

(2) The second method of turning up a flap or fasciculus allows of the fibres growing down the facial only (Fig. II.), and when they divide and multiply, as they actually do, they only supply one muscle, or one

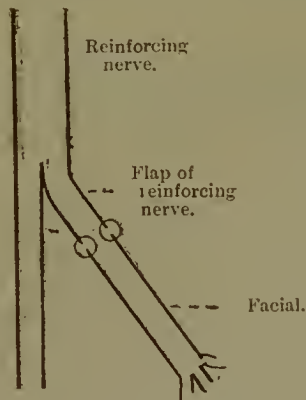


FIG. II.

group of muscles. In the spinal accessory three strands are visible, and I have used the middle one, separating it for about one inch from the rest of the trunk.

This allows of easy approximation without any tension at the suture line, and the power of elevation of the shoulder is very slightly weaker

than on the opposite side. This last factor is a little uncertain, owing to the additional variable supply of the trapezius and sternomastoid by the cervical nerves.

In the case of the hypoglossal, it is not so easy to isolate a definite fasciculus, though a flap can be raised. This entails some damage to the adjacent fibres. The XII. nerve lies deep in the wound, and the force necessary to manipulation often leads to temporary paralysis of half the tongue, but this disappears in a week or two.

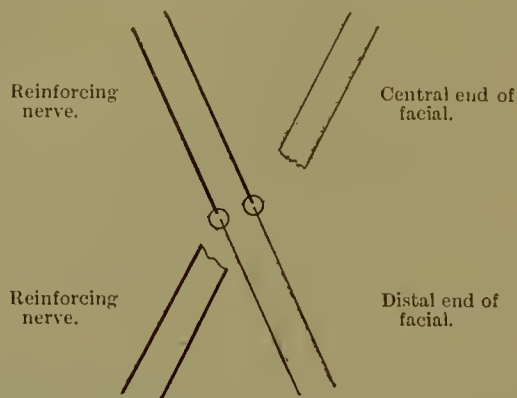


FIG. III.

(3) Complete division of the reinforcing nerve is unjustifiable in the case of the spinal accessory, owing to the shoulder and neck deformity resulting. It is the operation of election, I believe, when the hypoglossal is used. (Fig. III.)

The great advantage of this end-to-end anastomosis is the absence of any associated movements, and the ease with which co-ordination can be re-educated.

In the case of the XII. nerve hemiatrophy of the tongue occurs with at first some difficulty in speaking and swallowing. These pass off in a few weeks, leaving one-half of the tongue shrivelled.

The disadvantages of this, other than those of appearance, have been exaggerated.

Recently, I saw the hypoglossal completely destroyed by a small malignant growth, starting near the hyoid bone. The patient was actually unaware of any alteration in his tongue or speech. It is quite likely that the patient will prefer the disadvantages of end-to-side anastomosis to this, and the position should be explained to him.

Little need be said about the technique of the operation. The parotid should be pushed forward and not cut into two at all, and in nearly all cases the digastric has to be divided, and it is not necessary to suture it afterwards. Absorbable sutures of finest chromic catgut should be used, threaded on the smallest intestinal needle, with a full curve. I never use silk in any nerve suturing, as it is non-absorbable, and is surrounded by a small mass of fibrous tissue which interferes with the regeneration of down-growing fibres. It is usually recommended that the sutures should be passed through the sheath of the nerve only. In the small nerves the sheath is of the flimsiest description, and there is some risk of

these nerves coming apart. I have always used a single suture going right through the nerve substance. In the larger limb nerves, perineural sutures are possible and advisable.

During the operation it is helpful to use a weak Faradic current through sterilized electrodes. In this way it is easy to identify the nerves. This is most important in the case of hypoglossal, because in this region it is adherent to the vagus, and the two might be confused.

I have only once used cargile membrane in any nerve anastomosis, as it acts as a slight mechanical irritant, producing some scar tissue. It is necessary in cases where a lot of scarring is present already, because this is apt to extend between the nerve ends. This is occasionally found after extensive mastoid suppuration.

No restriction of movement is necessary afterwards, as no tension can be put on the sutures by turning the head. It is doubtful if subsequent electrical treatment is of any value, though facial massage undoubtedly is. When movement commences to return, exercises to improve the co-ordination and do away with any associated movements are essential. They are best performed in front of a looking-glass.

The first sign of recovery is that of improved muscle tone, noticeable when the face is at rest, next voluntary movement is possible, and last of all is the recovery of emotional action of the face. This takes some years, and it is said to be only perfectly attained in young subjects. It indicates the complete re-education of co-ordinating centres.

The results of this operation have been analysed in 22 cases by Muntz, 70 per cent. of the cases were successful, and this result will certainly be improved on by better technique. The opinion of this section is especially desired on the choice of the reinforcing nerve and the exact method of suturing.

The essential factor in anterior poliomyelitis is the destruction of certain motor cells in the anterior horn of the spinal cord. The fact that complete loss of power does not more often occur, is due to the fact that in nearly all cases the supply of a single muscle is derived from nerve cells, extending over about three segments of the spinal cord.

The first point to decide, as in facial paralyses, is the earliest time at which surgical interference is necessary and justifiable.

It is difficult to lay down definite rules; but I think if the case is properly managed from the first, it should be possible, with the aid of electrical reactions to say, within six months of the onset of the trouble, that a certain muscle will regain little, if any power. By proper management, I mean the relaxing of any paralyzed muscle, preferably on a splint, combined with daily massage.

With regard to the time limit for operative interference, it may be stated that tendon transplantation and anastomosis can always be used, but the possible improvement after nerve anastomosis rapidly diminishes as time goes on. Personally, I should hesitate to advise it after five years' duration, though Hackenbruch has reported a successful case after ten and a half years. My longest case, of nearly seven years' duration, where a flap of the healthy l.p. was used to reinforce the paralyzed E.P., was not a success. In a little over two years after operation, sensation had returned perfectly, but there was no power in the extensors and peronei muscles. The decision would be influenced, to some extent, by the amount of muscular contraction to the galvanic current in old-standing cases.

In many instances, it is optional to use tendon transplantation or nerve anastomosis, and the view of members on this point is asked for.

Taking first the subject of nerve anastomosis, it might be well to commence by disposing of certain cases which are unsuited for this operation. At the one extreme, are extensive paralyses, involving most of the muscles of a limb. The few remaining healthy nerves will be incapable of innervating the large number of paralyzed muscles, so as to give any useful result. On the other hand, certain cases recover with slight weakness in many muscles, but without loss of power in any one. Such are unsuited for any interference, for this is the condition actually aimed at after nerve anastomosis or tendon transplantation.

Occupying an indefinite boundary between these are cases where only a minority of the muscles are involved, and where there is a healthy motor nerve in the vicinity. I have had such instances, for example, in paralyses of the deltoid muscle, the flexors of the elbow, and the peroneal muscles of the leg.

The limits of this group will be decided after more extensive trials, and in some there will probably always be room for a certain difference of opinion. That there is a limit to this operation I have shown experimentally, in animals.

One small nerve can be made to innervate a large number of muscles so as to give voluntary movement, but so feebly that the limb is not a firm basis of support.

TECHNIQUE OF OPERATION.

1. As in the case of facial paralyses, it is always preferable to use a flap, or better an isolated fasciculus to reinforce the damaged nerve. For example, healthy nerve twigs to the long head of the triceps can be joined to the paralyzed circumflex or musculocutaneous in the end-to-end fashion. Twigs to the gastrocnemius are excellent to suture on to the paralyzed external popliteal. By this means, associated movements due to splitting of nerve fibres are impossible, and probably re-education of co-ordination is made easier.

2. In my earlier operations, I completely divided the reinforcing nerve, to take advantage of the multiplication of the nerve-axis cylinders.

After a flap is raised from a healthy nerve, the corresponding distal fibres degenerate, and multiplication of the remaining fibres would compensate for this. I have now given up this for several reasons.

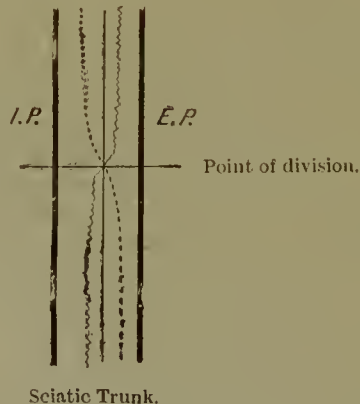


FIG. IV.

Partial division of a nerve, unless it be made shortly above where a definite branch comes off, causes degeneration of fibres scattered all through the trunk. Apparently the nerve fibres cross from one side to another of a nerve before arranging to form the fusciculi.

Partial section of the sciatic has been experimentally shown to be followed by a certain amount of weakness in all, and not a paralysis of a few, muscles.

After simple division and union of a nerve, Professor Osborne and myself have shown that few of the fibres regenerate down their old paths. Some of the motor fibres grow down sensory paths, and probably the reverse. (Fig. IV.)

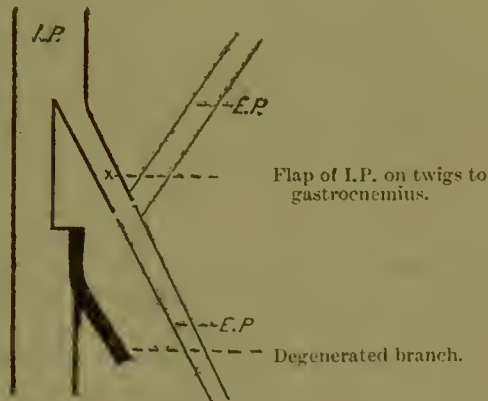


FIG. V.

This waste of fibres, and the difficulty in getting delicate co-ordination after nerve division, makes one hesitate to perform it deliberately. In addition, there are a certain number of cases which do not recover after simple nerve division.

I have been unfortunate enough to have had one myself, where I divided the reinforcing I.P. completely. Practically no recovery has taken place in the calf muscles. The explanation of this is presumably mild sepsis in the nerve suture, followed by scarring. The wound healed by first intention and there was no rise in temperature indicating any violent disturbance. A mild infection, which in almost any other operation would be unnoticed, is probably fatal in nerve surgery. These facts have led me to abandon complete division of the reinforcing nerve.

3. Sensory fibres are unimplicated in anterior poliomyelitis, yet these are so arranged in many mixed nerves, for example, the E.P., that they have to be divided at the operation. The reinforcing by muscular twigs to the gastrocnemius, gives comparatively few afferent fibres, so I have, in addition, joined central and distal E.P., forming a Y-shaped anastomosis. I am doubtful if this is absolutely necessary, but it is free from any objection. (Figure III.)

4. The sparing of sensory nerves to skin where possible.

In the case of the E.P., it is often possible to separate the fasciculi of the musculocutaneous from the anterior tibial, where only one of these is involved, and in any case the nervus communis fibularis should be spared.

5. In most of my cases, I have injected sterilized 2 per cent. eucaïne just under the nerve sheath above the level at which the trunk is to be divided. This blocks all sensory impulses, and the anæsthesia required is of the lightest. The resulting shock is insignificant, and children have been sent home after an operation involving division of both popliteals. If nerve blocking is not used, the anæsthesia has to be deep, but with chloroform given to the stage of fairly dilated pupils, there is comparatively little shock afterwards.

Nerve anastomosis in infantile paralysis must be done only on carefully selected cases. Cases of long duration will probably not recover; but we know too little at present to formulate any definite limit.

It is to be remembered that failure leaves the patient in a worse position than ever. Tendon transplantation, like nerve anastomosis, is only applicable where the majority of the muscles are uninvolved. It has the advantage of being possible many years after the onset of the paralysis, and the necessity and difficulty of making an early decision as regards operation disappears. There are very few cases suitable for tendon transplantation, to which nerve anastomosis is not possible. The two principal ones are paralysees of the extensions of the knee, and paralytic talipes valgus.

In the former case, if the sartorius is uninvolved, its implantation into the top of the patella, perhaps aided by slips brought forward to the knee-cap from the hamstrings, gives the best results of any tendon transplanting we know. Even in this case though, if the obturator nerve is intact, it can be sutured to the anterior crural, though I consider the tendon operation is the one to be preferred.

In talipes valgus, where the tibiales and perhaps the flexor and extensor longus hallucis are involved, nerve operations are almost impossible for anatomical reasons. To expose the damaged nerves, several deep and difficult dissections would be necessary.

I have treated three cases of this deformity by bringing over the peroneus longus from the outer side of the foot; and also in two cases splitting about one-third of the tendo achillis, and attaching it by silk strands to the periosteum of the under surface of the navicular bone.

A discovery of Lange, immensely extending the field of tendon transplantation, is that an interval of several inches can be bridged over by silk strands. The connective tissue condenses around them, and forms a dense tendon-like structure. On two occasions I have had to expose an area where I had performed bridging by silk some months previously, and have found the silk forming the nucleus of a firm tendon-like band. Several rules may be laid down for tendon transplantation.

A very careful electrical test should be made to accurately determine the paralysed and healthy muscles. It has been recommended to use synergic muscles where possible. Where muscles have to be sacrificed in the case of the leg, those acting mainly on the toes should be taken for the benefit of those moving and keeping steady the ankle and mediotarsal joints. In the upper limb, finger and hand movement is more important than forearm power.

With regard to the technique of tendon transplanting, I have found the previous application of a tourniquet renders the manipulations quicker and easier. It is said that implantation of a healthy into a paralyzed tendon results in subsequent stretching of the latter. I should like to get the opinion of members on this point. Personally, I have not used this method.

Though periosteal implantation, *i.e.*, direct fixation on to the bone perhaps after raising a flap of periosteum, is nearly always possible with the use of a silk bridge, in a few cases it is not possible to get to the full insertion of the paralyzed tendon. The tendons of the tibialis posticus and peroneus longus are notable examples of this and are all important in preserving the arch of the foot. Where possible, I have utilized the tendon sheath of the disabled tendon, as this seems to facilitate early restoration of function.

In some cases, it is advisable to use only part of the reinforcing tendon, as in cases where the tendo achillis is used to strengthen the peronei or tibial muscles. Elsewhere whole tendon is usually transferred, and sometimes an exchange is made suturing the paralyzed muscle to the healthy tendon.

The after treatment of this operation is most important, and consists in immobilizing the limb in plaster for about six weeks, to allow of the new attachment of the tendon becoming firm. Subsequently massage and exercises suitably graduated to develop the transposed tendons are essential. It is quite easy to overwork a muscle which is now called on to perform a new movement, by at once subjecting it to a strain which it is powerless to sustain. Graduated exercises would often induce sufficient hypertrophy for it to act successfully in time.

The most severe cases of anterior poliomyelitis are the bugbear of the surgeon. For their relief, we have the operation of arthrodesis, perhaps combined with tendon transplanting.

In these extensive paralyses in the leg, equally good results can be obtained by suitable apparatus, so that I am inclined to discard operative interference. In the leg the psoas and iliacus usually escape when all other muscles of the limb are helpless. This enables the leg to be moved forward in walking, and in cases where these muscles are involved, the leg can be swung forward with a little extra exertion by the pelvis.

To antagonize the genu recurvatum and unstable condition of the ankle, a suitable apparatus can be obtained with hinge joints at the knee and ankle permitting limited movement. This should be as light as possible. In poorer patients, a Thomas' knee splint ending in callipers fixed into the heel of the boot, or prolonged into a patten below the foot, is preferable on account of its cheapness.

Personally, I should not recommend arthrodesis of the knee in these cases, as the long rigid limb not only is apt to get in the way, but is liable to injury and fracture.

It may be advisable to perform it in the ankle joint in cases where all the muscles below the knee are helpless and the ankle joint is "wobbly." In the case of paralytic talipes calcaneus, Whitman has removed the astragalus and transplanted the peronei tendons into the os calcis with good result. In the upper arm type of infantile paralysis, the great drawback is inability to flex the elbow.

Some form of arthrodesis may be used to fix the elbow in the flexed position, because this allows of the hand being used in conjunction with that of the healthy arm. It is unnecessary, however, to perform the more severe operation at this joint, because the forearm can be permanently flexed by excision of soft parts above.

A diamond area of skin can be removed from the front of the elbow, and when the apices are sutured together, the limb is permanently fixed at the right angle position. This operation also has the advantage of being possible at any age, as it does not interfere with the growing ends of the bones.

These, gentlemen, are our resources in this distressing disease, and I should like to get your opinion as to the results you have obtained with them, and when they are individually indicated. This opinion is all the more valuable, as we have just had an epidemic of anterior poliomyelitis in our midst.

THE TREATMENT OF FACIAL NEURALGIA, WITH SPECIAL REFERENCE TO THE REMOVAL OF THE GASSERIAN GANGLION.

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The trigeminal, or fifth nerve, the seat of the mildest expression of neuralgia, or a suffering that baffles human endurance, has all the resemblance to a spinal nerve, and is of wide origin. "Its sensory part represents, from the third nerve to the hypoglossal (Starling), all the sensory roots of the motor cranial nerves." Its motor root has no place in the consideration of its neuralgic affections, and only becomes important when the question arises of trying to save it in removal of the ganglion. In the diagnosis of affections of this nerve, it is necessary to bear in mind the distinction between a merely minor neuralgia, that is generally curable by medical or minor surgical means, and that other form that becomes progressively worse, and finally leads to changes in the Gasserian ganglion itself. But, unfortunately, the distinction is not always easily drawn. The area of the distribution of the neuralgic pain is, perhaps, the best guide, it is not very definite, but up to a certain point it is the best available.

When the first division is the seat of pain, "it is found over the forehead, the anterior half of the scalp, the eyelid, eye, and side of the nose. Tender points may be found at the supra-orbital notch and at the outer side of the eyelid. When the second division is the seat of trouble, the pain extends over the cheek, between the orbit and the mouth, and to the ala of the nose. Tender points may be found at the infra-orbital notch, the side of the nose, and on the prominence of the malar bone. In affections of the third division again, the pain spreads over the parietal eminence, the temple, the ear, the lower jaw, and the tongue. The chief tender points are over the dental foramen, the auriculo-temporal branch, and others." (Taylor, *Practice of Medicine*, eighth edition.) A neuralgic pain in any of the above-mentioned parts may be caused by some local peripheral irritation, or be due to constitutional causes, in which gout, rheumatism, syphilis, and many other abnormal constitutional states play a part. In addition to the constitutional state associated with the first or ophthalmic division already mentioned, the influence of astigmatism, hypermetropia, glaucoma, iritis, and other local conditions must always be borne in mind. There is no doubt at all as to the influence of these states in causing neuralgia of this particular nerve. Uncorrected errors of refraction have been found during late years to play a most important part in the causation of neuralgia and other distressing affections of a nervous character. In regard to this affection connected with the two

remaining divisions of this nerve, it is unnecessary to point out the importance of a careful examination of the mouth, pharynx, and nose. The teeth will, in many cases, furnish information of the most valuable kind, and it is wise not to forget the influence of a wisdom tooth at a certain age. But, after all, we are in many cases forced to treat the minor neuralgias of this region symptomatically, as no cause, in some cases, after the most careful investigation, can be found. This region offers perhaps a wider field for the removal of causes than most, and, with this exception, the treatment of its minor maladies does not differ materially from the treatment of neuralgia elsewhere. But the presence of a neuralgia here has a very special importance, since it may be the beginning of epileptiform neuralgia. This condition has also been known under the names of *tic douloureux*, *prosopalgia*, and *major neuralgia*, a state that causes unheard-of suffering not equalled by any affection of the whole nervous system in intensity. The early recognition of this affection, no less than the limitation of the nerve which is the seat of the disease, will materially help in influencing the severity of the surgical measures that are sure to follow. Whether a knowledge of the early changes that take place in the nerve would help us it is impossible to say. Whilst various changes in the branches of this nerve, as well as in the ganglion itself, have been noted, there is at the present day no well-defined pathology relating to epileptiform neuralgia. This condition is rarely or ever found in connexion with the ophthalmic branch, and to a certain extent this is helpful in locating the branches affected, in the first instance, of the other divisions. The advantage of this will be appreciated when it is remembered that the whole question of the propriety or otherwise of peripheral operations hinges on this. Although, according to Jonathan Hutchinson, junior, the pathology as just observed is unknown, according to Sir Victor Horsely, the condition begins as an ascending neuritis in the smaller branches until eventually the ganglion itself is involved. At any rate, the latter supposition furnishes a good working hypothesis. The former authority lays down some very simple and excellent rules for our guidance in the performance of peripheral operations. Operations of this kind on the first division are rarely required, but occasionally successful; of this I have had a limited but favorable personal experience. In regard to the second division, he considers, if the palatine branches are involved, an intracranial operation should be performed, if not resection of the infra-orbital branches, as far back as possible should be tried. My experience of this is unfavorable. Concerning the third or last division, neurectomy is comparatively easy, and may be tried without making much demand on the patient, but when its branches are involved, this is of comparatively little use. The chief interest and importance of neurectomies in this region undoubtedly centre in the second division. This may be attempted by three routes—the infra-orbital, the antral, and the pterogomaxillary, the latter being the one now usually adopted, and is known as Krause's operation. The operation is a most formidable one, whichever route is adopted, and is attended with a great deal of disfigurement, the antral route especially being attended with these disadvantages to a conspicuous degree. In the *Annals of Surgery* for 1903, an operation by an American surgeon named Storrs is given, which gives a good idea of the value of the procedure generally. The operation of neurectomy of the second division was performed on some twelve patients, and two remained free from the disease for a period of ten years. This is considered a fairly favorable result. It is easy enough to define what nerves affected should be a guide

to the performance of peripheral operations generally, but I hold that in most instances it is nearly a hopeless task to do so in practice. If I might venture an opinion, founded, I admit, on a very moderate experience, it would be that in all distinctly developed cases of genuine epileptiform neuralgia, the radical operation of removal of the Gasserian should receive first consideration. I have not yet seen a case of the disease cured by the peripheral operation where the second and third divisions were involved. Intimately associated with the radical operation is the fact that most of the patients have undergone such prolonged and intense suffering that they are in many cases (the majority, indeed) quite unfitted for an ordeal that will try their powers to the utmost. I do not think that the intracranial operation for the removal of the ganglion is much more severe than the peripheral operation on the second division, involving the removal of the sphenopalatine ganglion. The removal of the Gasserian ganglion is now rendered quite reasonably safe by Cushing's modification of the Hartley-Krause operation. I had two experiences of Rose's method soon after its inception, and I do not wish to repeat them. In bringing before you an operation already well known and often put in practice, I do so for two reasons chiefly. It is certainly the best operation for those to adopt who, like myself, may do this operation at rare intervals. In the second place, there are a few points that I wish to emphasize, and that may have some value for those even who do the operation fairly frequently. The temporal method is now almost universally adopted, and marks a distinct gain, no less than a great advance, on all former plans of operating. Cushing's operation is termed "The Direct Infra-Arterial Method," and is a modified temporal method of the operators above-mentioned. Kocher, the great Swiss surgeon, after referring to Cushing's experimental on the cadaver, describes the operation *in extenso*. In giving a brief description of the operation, I will allude to the various points which I think merit special consideration. It is to be noted that this method does not commend itself to an osteoplastic flap. I have operated on a good many cases requiring an opening in the skull, and I have never felt that the absence of a portion of the bone has been a drawback. Here such a flap would be a distinct disadvantage. For cosmetic reasons an endeavour might be made to avoid the nerve supplying the frontalis muscle and the corrugator supercilii. The incision marking out the flap is horseshoe-shaped, and its base corresponds to the whole length of the zygoma, the highest point being on a level with the top of the auricle. The incision cuts clean through the skin, which is detached and turned down. The temporal fascia is cut to the same extent, and turned down, and at this stage it is recommended that the incision be carried along the middle of the outer surface of the zygomatic arch, and the peristœum detached, and the arch severed with powerful forceps at each end. Care is taken in this incision not to injure the masseter muscle. The incision of the periosteum over the zygoma and its detachment will be found to be both troublesome and productive of much delay. If both ends of the zygoma are cut down upon with a knife, they can be at once severed with a pair of bone forceps, and the part turned down. The temporal muscle and periosteum are next divided in the exact circumference of the wound, and reflected. When this has been accomplished, the base of the flap, including the severed zygoma, can be turned down as far as is necessary. The careful preservation of this flap, by surrounding it with sterile gauze and packing it out of the way, is indispensable. A good deal of time can be consumed in this preliminary procedure in stopping the rather free hæmorrhage. If

the blood vessels are simply clamped, and the forceps allowed to remain on till the next step is finished, it will save unnecessary delay. There are different ways of opening the skull, and in experimental work, in using the trephine I have been struck with the ease with which the dura could be wounded in this situation. It is best, then, to use a chisel and mallet, and chip away a small piece at the thinnest part of the bone. A very small opening will do. With a pair of small De Vilbis' forceps, a small gutter can be at once made to admit the use of the large size. By means of this and a pair of suitable strong bone forceps, the whole of the bone circumscribed can be easily removed. At the base it is necessary to go quite down to the infra-temporal crest, or even lower, this being one of the distinctive features of this operation. With reasonable care, not the slightest wound of the dura or accident of any kind may be apprehended. The opening may be made just over the middle meningeal artery, on the great wing of the sphenoid, but the danger of wounding this vessel in this situation is infinitesimal. As soon as the bony opening is of sufficient size, and its base practically on a level with the middle fossa of the skull, the elevation of the brain covered by its dura and the middle meningeal artery is next proceeded with. Whilst an assistant is prepared to deal with the elevation of the brain with a suitably bent spatula, the dura mater is carefully separated from the base of the skull. This may be started with any suitable blunt instrument, but, once begun, nothing is better than the index finger covered with a layer of sterile gauze to give the necessary degree of roughening. This will be continued till the foramen rotundum is reached, when the separation is to be extended back to the foramen ovale.

As its name implies, this infra-arterial method is specially designed with the view of obviating any interference with the middle meningeal artery, and if the various steps have been carefully carried out, the ganglion may now be dealt with, and the former disregarded. But I hold that in no case is it wise to proceed with the operation without first securing this vessel. If the procedure gives ample space without dealing with it, comfort, safety, and increased space is at once obtained in tying it. To those who have done any experimental work in this direction, the facility with which this can be done will be apparent. The foramen spinosum, through which the meningeal reaches its intracranial position, is placed behind and external to the foramen ovale, and can be reached first. I would strongly advise this part to be made the first objective, the artery tied, and finally disposed of. If the dura is raised up to a certain extent, the artery will at once come into view, accompanied by its two veins. Its stretching can be so nicely adjusted, and the space available so large, that to clear the vessel, tie it, and cut it between two ligatures is nearly as easily accomplished as if it was outside. At the risk of repetition, may I be permitted to say that I think this step the most important in the whole operation. Up to this stage, the hæmorrhage is moderate and easily controlled by gauze pressure. With the foramen rotundum and the foramen ovale and their accompanying nerves, in view, the next step is to liberate the ganglion. The Gasserian ganglion lies in the *cavum Meckelii*, which is a space on the floor of the middle fossa of the skull, which lodges it and its second and third branches. The various forms of recommendations given by various authorities how to clear the ganglion preparatory to its removal would leave one in doubt as to how it ought to be approached. This arises to a large extent from the meagre information we possess in regard to the exact relationships of this important space.

Recent works on anatomy contain no information that is useful, and but for the experimental work of the author of this operation, we might have remained till now quite unaware of its exact relations. It is to be remembered, then, that in addition to a covering of dura above and below, constituting the above space, the ganglion has a special fibrous capsule of its own immediately surrounding it. If the least mistake is made in opening this space, hæmorrhage of a most persistent kind is almost sure to follow, and this is just the spot where this is, if possible, to be avoided, and where it may quite mar the success of the operation. With the parts moderately on the stretch, a spot is selected on the inner aspect of the second division of the nerve, just as it enters the foramen rotundum. If a little opening is made here with a suitable dissector, the two layers of the cavum can be easily separated. This is continued to the foramen ovale, and then right back to the ganglion. When this is accomplished, the whole roof is quickly dissected back towards the median line, and the ganglion and its nerves are free on the upper surface. The same structures are then cleared from the floor of the space. When this is being done close to the foramen ovale, the bleeding is sure to be free, but is easily controlled. The two nerves are now divided at their foramina, and the ends seized with long slender forceps. By stretching the parts a little, the upper and internal part of the ganglion and the first division are freed. These manipulations must be conducted as closely to the parts as possible. With the parts still on the stretch, a division is made just behind the ganglion, and the latter, with its severed nerves, removed. It is extremely difficult to make any formal section of the ophthalmic branch, but on the removal of the ganglion this will be torn through. Some operators are so dexterous that they are able to leave the first division intact by a well-planned incision. It is during these final manipulations that the hæmorrhage may be particularly severe and annoying from a multitude of small ganglionic vessels. It is at this stage, also, where the risk comes in of opening the cavernous sinus. The blood-pressure in the sinus is so low that no difficulty will likely be experienced in stopping it. It is to be remembered, however, that bleeding here has proved fatal, and has led to the premature stoppage of an operation in the hands of most experienced operators. Gauze pressure at this final stage in the stopping of bleeding is singularly successful and prompt, and adds materially to the successful completion of the task. On section of the sensory root, at a certain distance behind the ganglion, fluid will appear which up till now has been absent. This is due to the tube of membrane in which the nerve is enveloped being opened, hence the appearance of subarachnoid fluid. It is a matter of no moment. With the removal of the ganglion and its branches, the operation is completed, the uplifted brain is allowed to return to its original position, and the wound is closed, drainage being adopted or not, at the discretion of the operator. On reaching the foramen ovale, it is sometimes found that a little growth or spicule of bone is present, so close to the opening that it has to be chiselled away before the operation can proceed. This is fortunately easily accomplished, but, if unexpected, may be embarrassing. This is a slight sketch, although, I am afraid, a very imperfect one, of a method that has already proved of great value, especially from the easiness, safety, and facility generally with which it can be performed. It is a modification of the procedure by Cushing and Hartley Krause, who may be regarded as the chief exponents of this method of surgery in America. This method requires no special experience or any special apparatus, and may be per-

formed by a surgeon just as he would perform any other operation. Just in proportion as this fact is recognised, so will operations be done at a much earlier period, and the general results achieved be much better than at present. So low is the vitality of most patients now submitted to this operation that, but for their intense and constant suffering, one would scarcely wish to consider the question of operation at all.

EXCISION OF THE GASSERIAN GANGLION BY A MODIFICATION OF THE ORDINARY ROUTES.

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My experience of this operation for the treatment of trigeminal neuralgia is limited to ten cases, and, as I successfully completed the last five cases by a slightly different route to that laid down by Rose and Krause-Hartley, I shall demonstrate to you on a skull my method.

The Krause-Hartley route, which I adopted in two cases, seemed to me to be an unnecessarily long way to go in order to reach the apex of petrous portion of temporal bone. The lifting of the lobe is a most difficult matter, and the hæmorrhage in my cases and also those of other surgeons with whom I was present, set me thinking that some easier way could be devised.

With Rose's method I have had three experiences. I found the operation terribly difficult; I followed his description in every detail, and the shortest time occupied was two hours and three-quarters.

It seemed to me that one could devise a route between the two mentioned. Taking Krause's base line as my starting point, I opened the skull there with a small trephine, and with bone forceps cut my way to the foramen ovale. The next step was to secure the nerve—the inferior maxillary branch—as it came out of the foramen; this was done by passing a blunt hook round it. A ligature was then tied round the nerve and given in charge of an assistant to pull on when necessary. At every foramen there is a tucking in of the dura mater. The space between the foramen ovale and foramen rotundum is crescentic, formed by these little processes. I found by dividing the dura between these points that I came directly on to the ganglion; my assistant pulling on the ligature on the nerve making this very clear. The next and final step is to remove the ganglion; this is best done by nipping it off piecemeal with sharp, curved scissors. I tried evulsion in one case and injured the cavernous sinus, which necessitated plugging the wound, and the patient—a very debilitated old woman—died fourteen days after the operation from a secondary hæmorrhage, which my house surgeon could not control. My operation, then, is as follows:—

- 1st step.—Removal of zygoma and coronoid process of the jaw.
- 2nd step.—Trephine at a point on the skull on a line with zygoma, then cutting away the bone between the trephine opening and the foramen ovale.

3rd step.—Secure the third division of nerve with blunt hook and tie a ligature to it.

4th step.—Divide dura mater from foramen ovale towards foramen rotundum, thus exposing ganglion.

5th step.—Cut away ganglion bit by bit, taking care to avoid wounding internal carotid artery or cavernous sinus.

I need hardly say that the strictest asepsis must be enjoined.

You will see on examining a skull that the distance from the foramen ovale to the ganglion is barely $\frac{3}{4}$ inch, while the distance from Krause-Hartley's base line is nearly 2 inches.

By the method I advocate, I have on one occasion removed the ganglion in 40 minutes, assisted by my house-surgeon—Dr. Langley. Dr. Hearne administered the anæsthetic (chloroform), and Dr. Mackeddle very kindly made microscopical sections of the ganglion for me. Dr. Fleming, of St. Arnaud, who sent me the case, reports that she has been quite free from pain ever since, which is now nearly two years.

DISCUSSION ON NERVE SURGERY.

DR. FIASCHI (Sydney) referred to the cases of nerve suture shown by Dr. Moore. He had used Cargile membrane to wrap round the nerve, and had not found it cause irritation.

DR. H. L. MAITLAND (Sydney), had operated on four cases of trigeminal neuralgia. In one he removed the Gasserian ganglion, in two he divided the fifth nerve intra-cranially, and in the fourth he removed the second and third divisions of the nerve and part of the ganglion by a method somewhat similar to that of Mr. O'Hara, dividing the zygoma, and opening the skull low down. He had tied the external carotid first, and had no hæmorrhage. He kept the head elevated, and temporarily clamped the common carotid, so lessening venous bleeding. The objection to the Hartley Krause method was the dangerous injury inflicted in the brain, which was avoided by Mr. O'Hara's plan. By removing only part of the ganglion, he avoided the risk of injury to the cavernous sinus.

DR. FIASCHI, Junior, had seen Hartley operate for removal of the ganglion many times. Ganglion symptoms never followed, and hæmorrhage was avoided by rapid operating. He had seen two cases in which the Abbé operation had been performed. Symptoms recurred, and on operating again it was found that the nerves had re-united, and the rubber tissue had disappeared. Mayo had suggested plugging the foramina with dentist's gold.

DR. DUNCAN doubted if Mr. O'Hara's operation would give sufficient space.

SURGICAL TREATMENT OF EXOPHTHALMIC GOITRE.

T. P. DUNHILL, M.D.,

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In this paper I do not propose to traverse the literature of the subject, and draw conclusions from the various publications. Rather, I want to discuss my own actual experiences. I have, during the last thirteen months, performed 57 partial thyroidectomies, and seen a number of cases upon which I have not operated, 47 of these have been on exophthalmic-goitre patients. During that time, watching the cases before and after operation, certain conclusions have forced themselves upon one. It is these conclusions, which may be only more or less correct, but which are built up from successive observations, that I wish to discuss with you, and on which I invite your criticism.

At the outset, I wish to use the term thyrotoxicosis instead of exophthalmic goitre, for the reason that in some of the worst cases there is no exophthalmos.

The question, should thyrotoxicosis be treated surgically has, in the majority of my cases, been answered by the patients themselves. Every case had been treated medically until it had become apparent to the physician or the patient that improvement was not taking place. In many of the cases, the condition of the patient had become extremely critical. At least two were *in extremis*. In cases which are definitely not improving under medical treatment, there can be no question as to the necessity for surgical interference if one believes that the disease is a thyrotoxicosis, and that the removal from the individual of a large part of the poison-producing body will enable the individual to regain and maintain a state of equilibrium as regards the output of thyroid secretion.

If one believes that successful operation will cure the condition, the question then arises, "Is the operation sufficiently safe in itself to justify one in recommending a patient to undergo it"? While not for a moment minimising the risks of the operation, which are very great and may easily lead to disaster, it may be said that the operation is as safe as any major operation in surgery. I believe that there is no need to fear death either at or following operation, provided the operation is performed under local anæsthesia.

In a large number of cases, one begins to notice differences in degree and type, with corresponding differences in character and rapidity of response to operative treatment. I have begun to classify cases of thyrotoxicosis as follows:—

- 1st. Those cases which have all the classical signs, viz.—goitre, exophthalmos, tachycardia and palpitation, tremor, but which are still able to do work, though with difficulty. Their tachycardia is constant, but palpitation only comes with a little extra exertion. They may look well or be greatly emaciated, but they have no organic heart lesion, no œdema. Frequently there is complete amenorrhœa. An individual case may suffer much with diarrhœa. The appetite is usually ravenous. I have operated upon 23 cases of this class. In eight of them I have removed portion of the second lobe subsequently.

2nd. Cases which have all the classical signs, but the symptoms have progressed until organic changes have taken place in the heart muscle, or there is dilated heart with inefficient valves. The patient has irregular intermittent pulse. Oedema is present, perhaps only a little on walking about, perhaps constant in whatever part of the body is dependent. In one case it was also marked in the face, on the side which was lowermost during sleep. This had been present for some months. In other cases, there has been simply oedema of the feet and legs on walking; the oedema disappearing on lying down. I have operated upon five cases of this class, on two of them I have removed portion of the second lobe subsequently.

3rd. Cases of incomplete development, but often with great toxæmia, the "formes frustes." In four of my cases the enlargement of the thyroid gland has been small, but associated with that there has been very marked tachycardia, and a constant sensation of smothering. In these four cases there has been no exophthalmos; characteristic tremor was present in each of them. One cannot call this class of case "exophthalmic goitre," the term "thyrotoxicosis" is much more suitable.

4th. Cases of what has been described as secondary exophthalmic goitre. One comes across cases which have been in existence a long time, and from the history it would appear that the goitre preceded the thyrotoxic condition. The thyroid gland is usually very much enlarged. The enlargement is often central or unilateral. If the patient be put to bed it is found that tachycardia is not always present, but palpitation supervenes so constantly on the slightest exertion that the patient is practically invalided. From the size of the goitre, and the long period it has been present, there is generally some pressure on the trachea. This causes dyspnoea on exertion and often during sleep. Indeed, it frequently prevents sleep. There is sometimes some prominence of the eyes, and there is frequently tremor.

I said there was a difference in the character and rapidity of response to operative treatment in these four classes.

Cases of the first class respond rapidly and almost completely to operative treatment. They are cases which have not lost their recuperative ability. Though they are ill, and though they have all the classical symptoms of the disease, they have a reserve of strength left, so that when the poisoning body is removed they at once feel a tremendous difference in themselves, and in a few weeks are almost as well as they ever were. I say almost. I might say completely. One woman says she can now do all her household washing and ironing, but she cannot do the scrubbing without some slight return of the palpitation. Nothing short of scrubbing ever causes it. Another cooks for her husband and two children, does all the household work, and, as well, teaches six music pupils. Two others are farmers' wives—one at Inverleigh, one at Koroit—each does the housework and a great deal of work incidental to the farm as well. These two had been completely invalided. I could multiply instances. This is the stage at which the operation should be

performed. At this stage practically complete recovery can be promised, and the period of convalescence is short. Ten cases were cured by one operation. Eight others were cured after removal of portion of the second lobe. One has not improved to the extent I should like, though she is definitely improving. One died. Three others were very recently operated upon.

If the patients have gone beyond this stage, and belong to the second class—if there are objective signs of heart failure—the rapidity of cure varies in time, and the completion varies in extent. All the text-books and articles which I can find speak of this class as being too far advanced for operative interference, presumably for two reasons. Firstly, because the patients are too ill to cure, and, secondly and chiefly, because the operation in such cases would be too dangerous. The statement that they are too far advanced for operation simply is not true. I would go so far as to say that no case, unless almost moribund, is too far advanced to obtain immense relief from operation. The extent of relief is an inverse ratio to the extent of the organic heart lesion. Under local anæsthesia, the danger of the operation, even in most advanced cases, is practically nil.

It is really most difficult to tell sometimes whether or not there is actually organic heart disease; I have seen a case with œdema of the feet and legs, with the apex beat outside the nipple line, with markedly irregular pulse, with eyes so prominent that she had twice had corneal ulceration, and who had the greatest difficulty in getting to town from the Western District on account of the œdema and dyspnoea. This patient left the hospital with a regular pulse of 80 per minute. She is a farmer's wife, who now does her own housework, and she writes me to say that she always feels well. One would have felt certain that this woman had myocarditis with a permanently damaged heart. One cannot expect this response in every advanced case.

The most that cases in this class can hope for is great improvement in their symptoms, without absolute cure. It is quite worth operating to obtain this result. Great relief can always be promised. Local anæsthesia is essential. A good example of the relief of symptoms is a nun on whom I operated. She had been under constant medical treatment for three years. All this time she had got steadily worse. I was asked to see her at her convent. Her face was then quite asymmetrical with dependent œdema on the side on which she had been lying. Her lower limbs were œdematous. Her pulse was most irregular and very rapid. She had a very large three-lobed goitre. She suffered with intense neuralgia pains at the apices of the lateral lobes and up into the ear, which prevented sleep. I might detail the other symptoms, but they were the usual symptoms of a very bad case of exophthalmic goitre. It was the œdema and the pulse which gave the interest to the case from the stand-point from which we are now viewing it. At first I had not courage to operate on this case. I kept her in hospital three weeks, and then sent her back to her convent. In the meantime I operated on another nun from the same convent who had been very ill with goitre for 22 years. Then the first nun, recognising the risks of operation, wished to be operated upon. I removed the right lateral and the middle lobe under local anæsthesia without difficulty, and the patient was never once in danger of life as the result of the operation. What I want to point out is the beneficial result—even in cases as advanced as this one was. Needless to say, before operation she was completely invalided. Since operation she has taken fairly long

walks, and has had a trip to Tasmania, visiting several convents there. The pain in the ears and upper neck region has disappeared. Her pulse, which was 120-130, is now in the 80's. It is still irregular, but quite quiet. There is œdema now, only after long walks, and only in the feet. With a little care she lives a happy life. Of course, she cannot take liberties.

One sees a number of cases in which there is a slight degree of œdema of the feet, but in which the heart beats regularly, though fast. Cases in which there is some acute dilatation, but apparently with no gross organic change in the myocardium. These cases recover practically completely, but the period of convalescence is more prolonged than in cases in which there is not cardiac dilatation. A good instance of this type is a lady who saw me in May of this year, and who had arranged then to be operated on. She was quite a typical case. On her way to Melbourne for the operation she heard of a case that had died after operation under general anæsthesia. My patient at once went back to her home in the country and stayed there for three months. She then came to Melbourne again because she was getting rapidly worse. She could not then get up the steps to my room. Her feet were œdematous. She had delirium cordis. She was very ill. I operated, removing the right lobe, the isthmus, and the lower pole of the left lobe at one operation. Her pulse slowed down, and all the objective signs improved, but she kept very weak and sweated profusely for six weeks before she felt much improvement. Then in the sixth week she gained 4 lbs. in weight. All the sweating stopped, and from that time on improvement has been continuous. She has gained $1\frac{1}{2}$ lbs. a week for the last four weeks, and is still gaining weight. I have operated upon five cases in this class. In two of them I removed portion of the second lobe subsequently.

The third class of case I regard as being the worst type to deal with. Small thyroid glands, no exophthalmos, marked tremor, intense tachycardia, constant smothering sensations. This type indicates that thyrotoxicosis is a dysthroidea and not a hyperthyroidea, an altered secretion, and not simply an excessive secretion. The symptoms are out of all proportion to the enlargement of the gland. In this class, removal of half the gland does not give the certain relief of symptoms that it does in typical cases. It seems as though even a small remaining portion of gland tissue pours out a secretion of such virulence that it is still able to poison the nervous system.

This class is really very small. I have operated upon four such. Of these four two are, after operation, able to undertake work as general servants, one other shortly will be able to do so. The fourth represents the only case which has not been relieved by operation.

The fourth class, described by some as secondary exophthalmic goitre, consists of cases in which the nervous symptoms are secondary, not primary. The goitre has preceded the heart irritability. As a typical example of this class I would instance a nun in whom the goitre had been present 22 years. She had had constant medical attention. During the latter years she was practically invalided through palpitation, tremors and sleeplessness. She was very frightened of operation, as the severity of the operation had been frequently pointed out to her. A large goitre was removed. She was out of bed on the sixth day; within three months she was managing, and is still managing, a large orphan asylum. I have operated on three cases of this nature. The cure in every case has been absolutely complete and very rapid.

The question of when to operate is answered by different practitioners in different ways. Every case should be treated medically for some time, and if it shows improvement which is continued, and shows definite signs of going on to complete cure, it should be persevered with. I have three such cases—out of some sixty which I have seen. In these three I have not recommended operation, although the patients placed themselves unservedly in my hands. But a practitioner who advises against operation in any case which is not gaining ground, or which is losing ground, is undertaking a big responsibility. Nor is the operation to be regarded as a last resort. If a patient is kept under medical treatment until it is obvious that she is going to die unless some miracle is performed, it means that that patient's chances of an ultimate complete recovery have been taken from her. The time to operate is as soon as it is recognised that the patient is not improving, and before she has lost so much ground that it will mean nursing her back to health after the cause of the disease has been removed. At the same time it should be clearly recognised that it is almost never too late to operate, and that the operation in itself need never prove fatal if performed under local anæsthesia.

In my opinion, local anæsthesia governs the whole question as to the safety or otherwise of the operation for thyrotoxicosis. The question is frequently asked: "Why use local anæsthesia, which may be imperfect, if the patient is well enough to stand general anæsthesia?" The causes which underlie death at or after operation are too ill-defined for us to be able to tell which cases will, and which will not stand general anæsthesia. Under general anæsthesia the operation cannot be recommended as perfectly safe. Under local anæsthesia it may be recommended as perfectly safe—matterless how ill the patient, unless moribund. In 47 exophthalmic goitre operations I have had only one death, due to causes which will be explained later, and which need not occur again. I know of three deaths which have taken place during the last year in cases operated on under general anæsthesia. There is no need for death to occur at or immediately following operation. Short of death, there are many reasons why local anæsthesia should be employed. Careful as one is to avoid the vicinity of the recurrent laryngeal nerve, on one occasion the patient suddenly lost her voice and could only whisper. I at once removed all the artery clamps I had applied, and in a few moments the patient spoke naturally. I believe that if the patient had been under general anæsthesia she would have had a silk ligature tied tightly around the nerve. In this instance the forceps had been applied to stop some deep venous oozing, but the nerve had evidently not been badly crushed. The patient is kept talking all the time while clamps are being applied, previous to division of the vessels. This saves one from cutting the nerve.

Another reason against general anæsthesia is that it is usually followed by some vomiting or retching. This is extremely painful with a deep and extensive wound in the neck; also it is not free from danger, through the possibility of secondary hæmorrhage from the large and brittle vessels which have been ligated. With local anæsthesia there is not vomiting.

Should there be any tendency to acute thyroidism, the treatment is to saturate the patient with fluid; preferably by copious ingestions of water. This is impossible immediately after general anæsthesia. Although the cases now never suffer from acute thyroidism, I always insist on their drinking three pints of water within the first two or three hours of operation as a prophylactic. Size of the goitre is no bar to its removal under local anæsthesia. I removed a goitre (not exophthalmic) from one

patient which weighed one and a quarter pounds, and that only represented one lobe and the isthmus. It spread widely under the sternomastoid muscles, displaced the carotid arteries and jugular veins, distorted the trachea, and extended apparently to the base of the skull. The total number of partial thyroidectomy operations that I have performed under local anæsthesia is 56. It has never been necessary to give a general anæsthetic in any of the cases. The anæsthesia has usually been exceedingly good, though a few have felt some pain while the deep connexions of the gland were being divided. This part of the operation takes a very few minutes.

Even should it be admitted that general anæsthesia might be used in selected cases, the majority are too far advanced for a general anæsthetic when they are sent, or come themselves, to a surgeon, and there are many—all of those constituting the second class mentioned above—to whom it would be impossible to administer a general anæsthetic—it would be courting disaster.

The next question is how much gland substance should be removed? The answer is: enough to cure the patient. Heineck quotes:—"The surgeons that have, for the cure of this disease, removed the largest quantity of thyroid tissue short of its entirety, are those that obtained the very best results, both from the standpoint of the number of recoveries, as well as from the standpoint of the completeness of recoveries." Yet too much should not be removed at one operation, for several reasons. Firstly, because it is bad surgery to remove more than is necessary to effect a cure. More can always be removed, but if too much be removed it cannot be replaced. Moreover, a second operation is safer even than the first, and the patients are so much improved as a result of the first, that they face a second operation with equanimity. Secondly, by removing too much at once an operation which is safe and moderately easy is rendered exceedingly dangerous.

There are differences of opinion as to whether it is necessary to remove more than one lobe. Kocher says that removal of one lobe and ligation of one artery of the other side is sufficient. C. H. Mayo does not advise removal of more than one lobe and the isthmus. Hartley and Halstead remove considerably more. In most cases the lobes are so large that if one lobe and the isthmus be removed, the remaining lobe is still very much larger than the whole of a normal gland. Also it is excessively vascular and active. In a case like this the removal of one lobe improves the condition much, but the gland tissue remaining is still sufficient to poison the individual, causing some tremors and palpitation on exertion.

If the patient is to be completely cured part of the second lobe must be removed. There is no danger of myxoedema if the upper and posterior portion of one lobe be left with its blood supply intact. The removal of portion of the second lobe has cured the condition in every typical case but one, and in that case I purpose removing some more gland. I certainly think that some months should be allowed to elapse between the first and second operations, because in many cases palpitation after the operation is simply the result of weakness, and disappears with the returning strength of the patient.

Next to the question as to whether these cases should be treated surgically, the chief interest centres around several points in the technique, chief of which is the method of division of gland substance. Almost all literature states that the gland should be crushed before division, and that the crushed portion should be ligated or sutured. This presumably for two reasons. Firstly, to prevent hæmorrhage, and secondly, to prevent the exit of thyrotoxic secretion.

If the operation is for the removal of one lobe alone, it does not matter much whether the isthmus be crushed or not, though even in this case it is quite unnecessary. There is nearly always a more or less fibrous division somewhere through the isthmus. This is easily found, and if the isthmus is divided through it there is no bleeding. Also crushing even across the isthmus causes an increase in pulse rate for some days after operation. Stitching up, or tying up the crushed isthmus, appears to me to be wrong, because it shuts up the crushed and bruised gland in a closed bag from which it has got to be absorbed. It should be treated as an abscess or an accumulation of any toxic material would be treated, that is, by simply allowing means of free exit to the exterior of the body for a day or two by means of a tube. This is shown much more definitely in the removal of half the second lobe at a subsequent operation. In my earlier operations on the second lobe I used to crush across, and attempt to ligate, because the literature had taught me to fear hæmorrhage when cutting across the middle of a gland in exophthalmic goitre. If a lobe is crushed across I have always found the patients exceedingly ill and in the gravest danger for some days subsequent to operation. This is from acute thyroidism, and I know now that it is because so much gland tissue is bruised and crushed and is licked up by the veins and lymphatics for some days afterwards.

In the whole series of 47 operations for the thyrotoxic variety of goitre I have had one death, which I believe was due to this cause. The case was my thirteenth. I had just begun to recognise that, in a large bilateral vascular goitre, the removal of one lobe was not enough to cure the case. The disease being a thyrotoxicosis, the large vascular lobe which remains is still larger than a normal thyroid, and pours out thyrotoxic material sufficient to cause all the symptoms of exophthalmic goitre. I therefore did two things, which I have since seen to be quite wrong. I removed the right lobe, the isthmus, and half the remaining lobe, at the one operation. This was the first error, and I crushed the second lobe across before dividing it; this was the second error.

Many cases since have been much worse before operation, but not one of them has caused me any anxiety. I remove one lobe at a time, and the patient stands it well. Later, if necessary, I remove half the second lobe, and again patient stands it well, but I never crush. Books speak of bleeding as the bugbear of cutting across a lobe, and they speak of crushing as being the safeguard against bleeding. If one crushes across a lobe, one must leave a large quantity of bruised and pulped thyroid tissue, which is licked up by the blood and lymph streams, and causes the gravest condition by acute thyroidism.

My opinion is not formed from that single case, but from the fact that all cases in which I crushed were very ill, and conversely that in a large number of subsequent cases I have sliced a lateral lobe from upper to lower pole without crushing, and there has never been the slightest danger from acute thyroidism. I have on the table before you several glands on which the extent of raw surface left exposed can be seen. A tube or piece of gauze was inserted in these cases for drainage purposes, and the neck wound closed around it. The amount of colourless secretion discharged on to the dressings from the large cut surface of gland is amazing, and it continues for four or five days. It is from the gland because it does not occur when a single lobe is removed through a fibrous isthmus, and it is constant when a large lobe has been sliced across. Yet this is not accompanied by symptoms. I think this is good

evidence that thyroidism is due to absorption of thyrotoxin from crushed gland, by veins and lymphatics of the thyroid and its capsule; and not by the tissues of the neck. Two other cases are most interesting in this respect. In one case while the gland was draining externally for ten days, the patient was exceedingly well and the pulse was 80. When drainage ceased, and the wound closed, the symptoms returned. It was as though the gland needed a duct to carry off its superfluous secretion. If absorption took place by the neck tissues this case should have been very ill in the earlier days of drainage. (She subsequently improved much.) The other case was drained by iodoform gauze. Instead of draining, the gauze blocked the exit. It was an operation on the second lobe, and the lobe had been sliced from upper to lower pole, in order to leave the parathyroids intact. When I saw her on the evening after the operation she was perfectly comfortable, but on looking at the neck I discovered the gauze drainage blocked, and the neck quite distended with secretion. I moved the gauze, and the retained secretion poured out. It had been under pressure, but it had never disturbed the patient's equilibrium. On the table is the gland of this patient, which shows the extent of the cut surface. I believe that not crushing, and free drainage, govern the whole question.

Crushing is not necessary to stop hæmorrhage. A lobe may be cut across with impunity. I have done it many times. When operating on a second lobe I cut from the front of the upper pole, downwards and backwards to the back of the lower pole. That removes from half to two-thirds of the lobe, while it leaves the blood supply from the superior thyroid vessels intact, and it also preserves the parathyroid glandules from any possibility of injury—both very important points. The hæmorrhage looks most alarming at first, but the larger vessels are in the capsule and are soon picked up, while the bleeding from the gland surface is stopped fairly easily by sponge pressure and a little underrunning with catgut. Sometimes the capsule is very adherent to the gland substance, and it is most difficult to enucleate. Hæmorrhage is then troublesome.

Occasionally there are large irregular veins entering the gland from the capsule anteriorly or laterally. These are ruptured during enucleation, and it requires some trouble to secure their proximal ends.

Twice in the series I have, when removing a gauze drain on the second day after operation, separated a ligature from vessel. In each case the wound required opening up in order to secure the vessel. It is only in the largest goitres that division of the infra-hyoid muscles is necessary.

What I have written may appear to imply that the operation is easy and safe. It is not right that the severity should be minimized. All handling must be of the gentlest nature. Manipulations must be rapid. The vessels are increased in number and size, and they are very brittle. If one gets off the track, the hæmorrhage may be appalling and uncontrollable. If the operation is prolonged the bleeding is bound to be excessive. It is not necessary, however, for any of these accidents to occur.

Finally, I would plead for early operation, for then the promise can be given of complete cure. Again and again cases come like one that was sent to me last week. I had seen the patient last November, when she was not very ill; she would not undergo operation then. Last week she was sent to me for operation, with a pulse varying between 180 and 140, and with oedema right up her legs to her lower abdomen. I have kept her in bed since. Her heart has now no bruits. She is just a skeleton, but I will operate on her now, and I believe she will quite recover.

HYPERÆMIC TREATMENT OF ACUTE INFLAMMATION.

F. HINRICHSSEN, M.D. TOOWOOMBA.

We have been accustomed to regard disease as an entirely vicious process aiming at the destruction of human life. But is this idea correct?

Let me quote the words of a great surgical authority: "The processes of disease," he says, "are aimed, not at the destruction of life, but at the saving of it."

And again: "Its manifestations are the outcome of a natural effort towards cure."

Further:—"In reality it (inflammation) is a wholly benevolent process with but one purpose—that of protecting the body, and one aim—that of cure."

These words are contained in an inaugural address delivered before the Philosophical Institute of Edinburgh, in 1905, by Sir Frederick Treves.

That address did not refer to Bier's treatment of hyperæmia, nor did it even mention Bier's name. And yet these words might have been used by Bier himself, since they exactly represent the leading idea which induced Bier, about seventeen years ago, to initiate his hyperæmic treatment.

So we see two great men, each independent of the other, meeting on the same path—a path, as it seems, leading to new and promising fields of therapeutic research.

Bier argued thus:—"If inflammation is really a useful process, we should strive, not to suppress it, but, on the contrary, to assist it in its beneficial work of freeing the tissues of microbes and their poisonous products." Consequently, he condemns all antiphlogistic measures the sole purpose of which is to try and check the inflammation.

Bier's object in the treatment of acute infection is to increase the inflammatory process by inducing an artificial congestive hyperæmia.

This hyperæmia most truly imitates all the main manifestations of inflammation. It retards the flow of blood and widens its bed; it inundates the diseased part with a serous stream; it increases the emigration of leucocytes.

Passive hyperæmia is brought about by constriction and by suction; the technique of both methods has often been described, so I shall not detain you by redescribing them. I only want to emphasize the fact that the application of the constricting bandage is not always quite so simple as it may at first sight appear. In some cases, it takes considerable trouble to find out the right degree of constriction which, of course, should only retard, but not stop, circulation.

In former years hyperæmic treatment, being used mainly for tuberculosis of bones and joints, was far from being generally adopted; and, though some surgeons gave good accounts, others entirely rejected the method.

Having had opportunity, as assistant of Professor Körte, to observe a number of these cases in Berlin, I must state that I did not notice by the method then used—constriction for one to two hours daily for several weeks—any obvious curative effect.

But, to my great surprise, I observed that the patients did not complain of pain, although their limbs were swelling considerably; nay more, pains already existing were mitigated.

It is, indeed, a striking feature of this treatment that pain is in most cases lessened. One gentleman, suffering from gouty arthritis of the wrist, found this out for himself, and used to tightly clasp his forearm to relieve his pain. Now by means of a rubber bandage he attains his object in a more effectual way.

All this may strike us at first as rather strange, but, as the matter has been confirmed by many observers, there can be no doubt about its correctness. And, moreover, we may plainly admit that our knowledge of the genesis of pain is still very limited. There are, indeed, some theories, but they are not built on a very solid foundation.

However, after Ritter's investigations, so much we know—that the widespread doctrine that exudation causes pain by pressure brought to bear on the nerves can no longer be maintained.

Amongst other arguments, Ritter, who made interesting investigations on this subject, draws attention to the often-observed fact that in acutely-inflamed part sensation diminishes gradually with the progress of the oedematous swelling and the infiltration of the tissues. There are cases of inflammation with excessive swelling where the pain is less than at the beginning of the inflammation. This, of course, would be inconsistent with the idea that it is the swelling which causes pain in inflammation.

The condition brought about artificially by passive hyperæmia may in a similar manner influence sensation; and so the diminution of pain by constriction, which appears at first glance quite paradoxical, may be brought closer to our understanding.

Having acquired during many years of trial considerable experience with his new method, Bier found himself compelled to make wider use of it. He applied it, indeed, not only in chronic cases of tuberculosis and in gonorrhœic antritis, but also in cases of acute inflammation. And it is just in the latter category that passive hyperæmia leads to its best and most unexpected results.

Unfortunately, I have not had personal opportunity of making large and varied use of the method. But, though small my material, it will suffice, as I hope, to show that the purposely-induced hyperæmia is a therapeutical agent of extraordinary importance.

In the treatment of furuncles, Bier's method is superior to all others. Two cases of furunculosis will demonstrate this—

A healthy middle-aged man had been troubled for weeks by a dissemination of furuncles on the calf of the leg. Several had been lanced, but fresh ones sprang up again and again. When I first saw him, there were three big furuncles and several smaller ones. One showing pus near the surface was lanced by a small incision of $\frac{1}{6}$ th of an inch; no tampon was introduced. The suction-glass was then applied to all the furuncles, and a moist dressing was used. The small fresh boils quickly disappeared in one or two days without any formation of pus. All the furuncles were healed after eleven days.

Another man—a strong healthy young farmer—presented at the back of his neck numerous furuncles of all sizes, and in all stages of development. The neck was stiff and the cervical glands swollen. No incision was made. On application of suction-glasses for three-quarters of an hour, pus was freely evacuated. The procedure caused no pain worth mentioning.

After one sitting only, the patient was much relieved. Two days later he could move his head so well that he went to work. In another couple of days he returned with a new crop of boils. These were all quite in the initial stage, and were healed after two sittings.

From these and other similar observations I venture to draw the conclusion that by hyperæmic treatment of furuncles most speedy relief is given, together with a minimum of pain. Moreover, the healing process is abbreviated, partly owing to the avoidance of big incisions.

In a similar way I have treated three big subcutaneous abscesses, two of them caused by injection of saline into the axilla, and extending over an area of about 3 square inches; the third, a smaller abscess of the size of a peach, situated over the gluteal tuberosity.

The proceeding was to make small incisions of $\frac{1}{4}$ - $\frac{1}{2}$ inch, then suction every day till the wounds showed healthy granulations and there was no exudation of matter. A plain aseptic dressing was used. There was no introduction of drainage-tubes or gauze. Although these cases did not heal more markedly quickly (10-12 days) than usual, the patients reaped the advantage of being spared the painful handling by the introduction of tubes or gauze tampons.

Moreover, the small incisions leave only a tiny mark, which after a time can hardly be detected. This may sometimes be valuable, for instance, in the face and on the mamma.

The deep and hard scars remaining after a mastitis correctly cured by broad and deep incisions do not represent a cosmetic success, nor are they indifferent respecting lactation.

Here, again, Bier's treatment is a decided step upwards. Two cases of puerperal mastitis came under my observation, and were treated by means of suction-glasses.

In the one instance, a young nursing mother was highly feverish, and the outer half of the right mamma showed extensive hard infiltration. The skin over this area was dark-bluish, in parts nearly necrotic. A small incision was made under chlorethyl spray. Then, by suction-glass, a great amount of pus was evacuated, together with milk and some blood. The suction-glass was applied daily for three-quarters of an hour. On the eleventh day, the patient had to leave town. She was then perfectly well, and had no pain. The skin had partly sloughed; the wound was superficial, showing good granulations; there was no pus in the depth, and the function of the mamma was satisfactory.

In the other case a circumscribed subcutaneous abscess of the mamma was opened by an incision of $\frac{1}{2}$ inch in length, over 2 ozs. of pus being evacuated. After applying suction for eight days, the discharge had entirely stopped, and the wound was healed a few days later. The scar did not show in any way, and the mamma had its natural shape.

In cases of infection of sutured operation wounds, the method proves very useful. It permits one to dispense with a broad re-opening of the wound, and particularly of the sutures of the deeper layers which, as in cases of herniotomy, for instance, may be of decisive importance.

I have lately observed three stitch abscesses, two of them, after Bassini's operation, caused by buried silk sutures showing up in the second or third week after apparently primary union.

Only the skin wound was slightly opened; suction was applied; all healed up well. But one of them recurred after a fortnight, a fresh sinus forming. Suction was again applied, without, however, perfect success. It was finally necessary to remove one of the deep-buried silk sutures.

The two other abscesses were cured in a few days by suction only, leaving a firm fine scar like that seen after primary union.

The case I am now going to bring under your notice presents a special interest as to the curative effect of hyperæmia—

W.—a healthy country lad of thirteen years of age—was admitted to the Toowoomba General Hospital on the 9th of June this year. A fortnight previously he had met with an accident, having cut his right knee near the patella while wood-chopping. It was only a small wound, to which little attention was paid at the outset; but it soon began to suppurate. Ten days later, the knee was swollen and was extremely painful, causing sleepless nights. On the day of admission, the patient's temperature was 101 degrees; the pulse being 100-120. The general condition was not very bad. The right knee was considerably swollen, the grooves round the patella were filled out, and there was some fluid in the superior recess. There was an open wound, $1\frac{1}{2}$ inches long and $\frac{1}{2}$ inch wide, lined with bad granulations and covered with pus near the lower margin of the patella, there being no free communication with the knee-joint. The knee was fixed in slight flexion; every movement, or attempt of it, was very painful.

A puncture of the joint made by means of a hypodermic syringe showed pus, the microscopic examination of which revealed no microbes.

The treatment consisted in applying a rubber bandage at the middle of the thigh, the wound being simply covered with plain sterilized gauze, loosely bandaged to the knee.

Soon after the first application, the patient felt greatly eased, and began to move the knee. The temperature fell to normal on the third day after admission. On the fifth day there was scarcely any exudation to be found in the joint; the wound looked healthy, and there was no secretion from it. The constriction, which had been applied for 22 hours daily hitherto, was now continued for about 10-12 hours daily.

On the 27th June, the eighteenth day from admission, the patient left the bed; the function of the knee being perfectly restored. There was no exudation whatever, and the wound had healed up.

After the 30th June, the rubber bandage was entirely left off, and a few days later the patient left the hospital perfectly well.

Though I am well aware that under favorable circumstances a mild purulent infection of a joint may be cured by rest and suspension only, I nevertheless, in this case give the credit to the congestive hyperæmia, because no rest or any other antiphlogistic treatment had been resorted to. On the contrary, the patient was allowed, and even encouraged, from the start to move his knee in order to prevent the formation of adhesions, which are so often the cause of stiffness after prolonged rest.

On account of the mildness of the infection, it seemed in our case permissible to dispense with any operative treatment, such as puncture and washing out of the joint. Thus the case shows clearly and purely the effect of venous hyperæmia, confirming Bier's statement that a closed abscess may be healed under the sole influence of hyperæmia.

The treatment gives great satisfaction by affording almost immediate relief without forcing the patient (for the purpose of stillness) to take up a more or less uncomfortable position.

Only once have I had the opportunity of treating a gonorrhœal synovitis of the tendon-sheath at the dorsum pedis. Here I had some trouble to find the right degree of constriction. If the elastic band were but slightly too tight, the pain was much increased; but, at last, with proper application of the bandage, this patient too felt great comfort. The synovitis healed, though scarcely more quickly than usual.

The last case I have to mention concerns a girl who had been afflicted for several months with a troublesome superficial suppuration near the nail of one finger. In spite of numerous antiseptic applications in dry and moist form, the affection did not heal. By using constriction at her arm for ten days, the skin became perfectly normal.

May I now briefly sum up the advantages of the new method of treatment? They are as follow:—

The inflammatory process is shortened; often, even at the initial stage, inflammation is entirely suppressed; the treatment causes but a minimum of pain; the painful plugging and draining is avoided, and existing pain is mitigated. Since large incisions are often replaced by small ones, much disfiguration is avoided; and a small incision under local anæsthesia may be substituted for a graver operation under a general anæsthetic.

The most important point is the possibility afforded by the new treatment to save the function, even in such cases where formerly only a proceeding regardless of the function seemed able to check the suppuration.

Here the new method may be very beneficial by quelling an infection at its first stage, and particularly after all those operations in which the whole success depends on a first intention, as, for instance, in herniotomy and other plastic operations.

Further, this method allows one to deal with contused and infected wounds in a more efficient way, by suturing, for instance, severed sinews from the start, trusting that the hyperæmia will enable the tissues to fight successfully the microbic invaders.

A still more valuable advance has been achieved in the treatment of purulent synovitis of tendon-sheaths, of which, unfortunately, I cannot give you an example.

Very poor functional results obtained from older methods have, under the new treatment, been turned into the reverse. In 1906, Bier reports on 25 cases of this kind treated by hyperæmia. Of these, no less than seventeen healed without necrosis of the sinews and with perfect, or almost perfect, function. All cases which entered for treatment during the first seven days from the beginning of the disease gave a favorable result.

Were this the only feat achieved by the hyperæmic treatment, it would render the new method worthy of the earnest consideration of every surgeon.

In managing suppurative synovitis of big joints, both the formation of adhesions and the atrophy of muscles is hindered by the patient being enabled to move his limb from the beginning of the treatment.

Some disadvantages have to be recognised. The surgeon has to devote a greater part of his time to the patient, and has in some cases to watch him more closely. A wider surgical experience is required, because the œdema and the absence of pain disguise partly the state of the suppuration

at a given moment. This may lead to some difficulty in finding the proper moment when further surgical interference is needed.

With regard to harm caused by the treatment, several observers have noticed that the constriction bandage, if applied to limbs with varicosities, caused thrombosis. Further, slight infiltration has been detected at the lower peripheral margin of the rubber bandage, and, in a few cases, erysipelas has been regarded as produced by the constriction.

If constriction be too tightly applied, more harm may be done, even to the deeper parts. Thus, sloughing of muscles has taken place at the situation of the elastic band. But here the rules laid down by Bier had been neglected; the constriction was too tight. It should always be abandoned when the patient complains of pain.

Like all therapeutic measures, this method is bound sometimes to lead to failures; but irreparable harm is not done by these if sufficient attention be paid.

All agree that results improve with increase of experience, and with greater skill in mastering the technique. Failures have, however, been met with, even by most skilled men who were thoroughly versed in using the hyperæmic treatment.

The question why this happens—why some cases are refractory—leads directly to theoretical explanations. I do not intend to go into theoretical details, as the theory of the subject is still in its early stage. It represents, indeed, a complete chaos—a great variety of widely-differing opinions, each contending with the other, and all more or less unsettled.

I will not impose upon your time, nor try your patience, by going into these controversies. I want, however, to go so far into the theory as to show that Bier's treatment, though opposed to many an old and apparently well-established idea, is nevertheless based on scientific grounds, and built up by an excellent observer of nature and an independent thinker.

According to Bier, hyperæmia destroys pathogenic microbes or diminishes their virulence. This contention has been supported by Nötzel's experiments on animals, which have, in their turn, been confirmed by the experiments of others.

A dose of anthrax bacilli and of very virulent streptococci which, under ordinary conditions, had been proved to be without doubt deadly, had no ill effect whatever when injected into a part kept under the influence of hyperæmia. This exceedingly remarkable fact has given rise to a great deal of research. Investigations have been directed to all the different phenomena connected with artificially-induced hyperæmia.

The serum obtained from a hyperæmic limb has been examined by Laqueur in test-tube experiments, which proved this serum to have a higher anti-microbic power than that of normal blood serum.

But his statement has been contradicted by Baungarten, who attributes to the normal blood serum a higher anti-microbic power.

Others, especially Heller, point out that during congestive hyperæmia the products of the microbic metabolism are retained at the place of the inflammation, and that they prove detrimental to the germs.

Hamburger considers the greater amount of carbonic acid in the blood as the main bactericide agent of venous hyperæmia.

Further, the leucocytes have, of course, attracted much attention. They appear in greatly increased numbers at the inflamed and congested part,

and seem to perish here to a large extent—setting free during life, or by perishing, a ferment. This latter is regarded as having bactericide power.

Then, phagocytosis is increased, both from the presence of a greater number of leucocytes and from their greater capacity to take up microbes, which is supposed to be due to the presence of opsonins, or stimulins, or other bacteriotropins, in the serum of the congested limb.

So much of the theories on the anti-septic power of hyperæmia may be sufficient to demonstrate that modern scientific ideas are well consistent with the new form of treatment. And, in fact, those surgeons who do not appreciate hyperæmia as an efficient therapeutic, yet agree in admitting its bactericide power. Lexer, who stands in the forefront of these men, is even afraid that by the destruction of numbers of pathogenic germs such an amount of endotoxins may be set free as to endanger the organism by a sudden inundation of these poisonous products. Sudden rises of temperature, sometimes observed after removal of the constriction, are considered by him as caused by resorption of endotoxins.

Bier, on the other hand, maintains by good arguments that, in the state of hyperæmia, the tissues are able, in a manner not yet known, to neutralize the endotoxins, and so prevent any harm which might have resulted from resorption.

Another most important attribute of hyperæmia is its power of dissolving solid tissues. Bier has observed that hardening of sinews, adhesions of joints and keloids, disappeared completely under hyperæmic treatment. This dissolving power is certainly of importance also in the treatment of acute inflammation, inasmuch as it accelerates the liquefaction of tissues, so assisting the maturation of an abscess, or preparing the resorption of an infiltration.

Possibly, here again, a ferment originating from the leucocytes plays a rôle—a so-called proteolytic ferment which, however, finds a counter-agent in a so-called anti-proteolytic ferment contained in the blood serum and in serous exudations.

To what extent all these different principles constitute the curative agent of hyperæmia has to be explained by further observation and research. There are still many obscure points; but in some directions a satisfactory explanation seems possible, even at the present day. I refer to the increased serous transudation, which is certainly useful in forming an irrigating stream from the tissues to the periphery. By means of this serous stream, microbes and other contaminations are swept away from a wound, or from an inflamed part, when opened by incision. This guarantees a more efficient disinfection of the tissues than that possible by chemical or mechanical means.

I hope it will be evident from this account, short and incomplete though it be, that the phenomena of hyperæmia will find a satisfactory explanation by other facts long since familiar to us.

But whether the theory be built up sooner or later, practice has already demonstrated the extraordinary effect of hyperæmia.

How far-reaching this effect is has to be proved by further experience, and, particularly, whether deep-seated processes—osteomyelitis, for instance—and most severe cellulitis can be successfully combated.

Certainly in many circumscribed inflammations tending to formation of abscesses, and in all lighter cases, the new treatment has great advantages over older methods.

THE ASSOCIATION BETWEEN DENTAL CARIES AND APPENDICITIS.

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The subject of appendicitis and its causes seems to me fraught with the gravest interest in these days when the disease occurs so frequently.

There is no doubt, I think, that there has been considerable improvement in diagnosis; but no one will deny that there has been an enormous and lamentable increase in the incidence of appendicitis.

Great and important as I believe the increase to be, I have thought that we are as a body tending rather to overlook the all-important subject of etiology, in the surgical interest, which the disease and the measures for its relief arouse. It behoves us to fathom the reasons of its increase, and having done so to find some means of prevention.

I have looked up a number of authorities on the subject, and have read all that is written on the etiology of appendicitis in the medical literature at my disposal. I find mention of a host of different causes; but taking each cause separately, and considering whether it in any way explains why appendicitis is so common, I find myself forced to admit that none of them fully explains the matter.

Here is a list of some of the causes of appendicitis that I have culled from the literature at my disposal—

- Atony of the bowel.
- Catarrhal enteritis.
- Colitis.
- Typhoid.
- Scarlet fever.
- Dysentery.
- Syphilis.
- Tubercle.
- Obstruction to the outflow of appendiceal secretion.
- Kinks.
- Constrictions, contractions of its lumen.
- An inherent weakness in an organ said to be vestigial.
- Constipation.
- Diarrhœa.
- Dyspepsia.
- Traumatism from without.
- Traumatism by the *psaos* muscle.
- Foreign bodies; over-eating, &c.

None of the above seem to me to completely fill the bill.

But besides the above, there are certain microbes which, being found in diseased appendices and in the abscesses occasioned thereby, are to be looked upon as in some degree associated with the causation of the trouble. Constipation and dyspepsia, of all the causes enumerated, are the two conditions which go nearest to being as common as appendicitis. They are even more common.

Constipation, however, was a very common condition twenty years ago, and so far as my experience goes, I feel sure that not nearly so large a proportion of my constipated patients suffered from appendicitis in those days as do at the present time.

Independently of constipation, there are perhaps more dyspeptics at the present time than there were twenty years ago. But even the dyspeptics of twenty years ago did not suffer from appendicitis in the same proportion as that class of patient does at present.

It is quite safe to say that there is no case of appendicitis in which there is no dyspepsia present. The nature of the lesions in the appendix and cæcum makes it certain that there will be some form of indigestion. A large part of the dyspepsia accompanying appendicitis is certainly due to these lesions, and frequently it is actually concerned in the causation of appendicitis. Flatulent indigestion seems to be present in nearly every case at some stage or another, and certainly in some cases seems to suggest itself as the cause of the trouble.

While it is difficult to say to what extent dyspepsia has increased during late years, it is safe to admit that there has been some increase probably to some extent associated with the hurry and worry of life, the haste over meals, &c. But the conditions that have increased to something like the same extent as dyspepsia, and appendicitis, are the various diseases of the teeth.

Almost my sole object in writing these notes is to call your attention to the association between diseases of the teeth and appendicitis. If you can believe as firmly as I do in the frequent and intimate association of the two as cause and effect, then you will also agree that it becomes of national importance to discover the cause of the enormous increase of diseases of teeth, and find some preventive measures for them.

I have made an attempt to tabulate reasons for believing diseases of the teeth to be etiologically associated with appendicitis.

1. There has been an undoubted increase in both, almost *pari passu*, during the last twenty years.
2. Almost every patient with appendicitis has diseased or deficient teeth, generally the molars are at fault. More often the tooth trouble observed consists in some bacterial affection. Less frequently, there is deficiency of molars rather than bacterial disease.
3. Almost every patient with appendicitis will give you history of recent tenderness of teeth, a gum boil, an aching hollow tooth, or something that was tender to bite upon.
4. Among patients who have clean mouths and complete or almost complete sets of teeth, whether natural or artificial, appendicitis is rare.
5. Appendicitis and bacterial diseases of teeth are rare in infants up to two years of age.
6. At later periods of life, from two years of age and upwards, both dental caries and appendicitis increase till, say, the age of 30. People of 40 and 50, being born before the cause of deterioration in teeth came completely into operation, retain, even at that age, better teeth than the younger generation, and suffer less from appendicitis.
7. Flatulent dyspepsia is especially due to dental deficiencies, and more particularly to bacterial diseases of the teeth, and flatulence has a close association with appendicitis, both before and during the attack. It no doubt plays a large part in the causation of kinks in the appendix, which in their turn play such a large part in the causation of the disease.

8. Of the microbes mentioned as discovered in association with appendicitis, viz.:—*Proteus vulgaris*, streptococcus, staphylococcus pyogenes, aureus, bacillus pyocyaneus, all are found among diseased teeth. Typhoid and influenza bacilli have been found in the mouth and in the appendix. *Bacillus communis coli* is found frequently in appendiceal lesions, and it would be odd indeed if it were not, since it is normal in the intestines.
9. Aboriginal races of dark colour have good teeth, and rarely suffer from appendicitis.
10. The British races have deficient teeth, comparatively, and suffer from appendicitis to a greater extent than the black races.

Now, by this time, I hope I have been able to show at least that diseases of the teeth play a large part in the causation of some cases of appendicitis, and that the enormous increase in the former during recent decades largely explains the enormous increase in the latter.

To those who are prepared to admit these propositions, it will be no difficult matter to agree also that it is quite time that the matter of prevention was taken more seriously and generally into consideration than we have perhaps been doing in the past.

It is not very necessary for me to say anything to impress you with the great importance of the two diseases which I am endeavouring to associate. Their importance to us arises out of their frequency, and their effect upon the Australian nation. So frequent and so unexpected, and so disastrous are the effects of appendicitis, that it becomes a certainty that it is unsafe for any man who has once had an attack to betake himself to the back-blocks for his livelihood without previously having his appendix removed. Moreover, if the disease goes on increasing as it has done, it will one day become a question whether any man should go "out West" or "up North" with his appendix inside him, whether he has had a previous attack or not. You must, every one of you, have had to decide the question for those who have had an attack whether they should be operated upon or not before travelling. You must have all had to sound a note of warning, even when you were not directly consulted on the point. It is quite true that statistics of appendicitis show a fine percentage of recoveries in unoperated cases, but it must be remembered that the unoperated cases, upon which statistics are based, are those that occur in cities, and are within reach of careful attention, rest, and good physicians. They are, therefore, within reach of that which contributes largely to low mortality in this as in all other diseases.

In this connexion it should, by the way, always be remembered that diagnosis in unoperated cases must be more uncertain than in operated cases. This being so, there must always be some little shade of doubt upon the value of statistics upon this point.

Consider, now, what is our position in Australia as regards diseases of the teeth, and especially dental caries.

It is almost universal experience among our own children that dental caries begins to show itself even in their deciduous teeth. Little things of three and four years of age begin to suffer from toothache, an unpoetical and abominably painful complaint, even for an adult, let alone a little child. The pity of it is accentuated by the uncontrovertible fact that it is preventable if we only knew the cause of it. As the child goes on,

the trouble grows worse, so that it is only with the greatest care and attention, that adolescence or maturity is reached with any teeth remaining, most of those being filled with some material or another, of which they mainly consist.

It may be that the causes have been already sufficiently studied, and that among the many conditions suggested as causes of dental caries, that one at least is sufficiently established on sound evidence. If this be so, no remedy, no general attempt to provide a remedy, has been made in this country.

The subject deserves the most thorough consideration by surgeons and dentists. Laboratories, if need be, should be equipped, experts told off for the sole study of the conditions concerned, inspections of school children, and, if need be, experiments should be made in the feeding of State wards, &c., &c. Some serious attempt should be made to provide fathers and mothers throughout the Australian States with the information and instruction which will enable them to bring up their children with decent teeth.

I am, myself, at the present time, unable to find anything sufficiently definite to satisfy me in formulating advice for others. I am inclined to hold certain opinions, but for the most part, those opinions are based on evidence which would not avail in organizing a raid on the faulty modes of life concerned.

So far as the study of the micro-organisms of the mouth is concerned, it seems to me to establish certainly this much:—

That dental caries requires for the most part an acid medium for its development.

That such an acid medium is provided by a carbohydrate diet, leading to the formation of lactic acid.

That nitrogenous diet does not assist in the formation of the acid medium needed for caries, except so far as meats, &c., may act as culture media for bacteria, which later may produce an acid reaction in carbohydrate materials (Burchard).

General experience seems to teach the following:—

That meat-eating races show less dental caries than those eating a mixed diet.

Australian aborigines, before the advent of "civilization," were largely meat eaters, and had good teeth. "Civilized" aborigines have a much larger proportion of caries.

That the excessive proportion of meat, alleged, in the mixed diet of Australians, does not protect them from caries.

That South-Sea Islanders, who live chiefly upon carbohydrates in their island homes, came to Queensland with clean mouths and went away with dental caries. Living in their islands largely on carbohydrates did not give them caries. Mixed diet in Australia did.

It has been said that deficiency of lime in drinking water is largely responsible for dental caries, and certainly the few good mouths of teeth that one sees now are generally in middle-aged men or women who, in their youth, lived in the "bush," and drank from running streams. Drinking from running streams, no tank water at any rate, is common to all aborigines, and for the most part to those Australian-born, who have good teeth.

Roller-ground flour, the modern and more refined product of milling, is held responsible by some for the increased incidence of caries. It appears that in the modern process, much more of the shell of the grain, and with it, more of the diastase which assists in digestion of the starchy interior of the grain, is removed than was the case in the old stone-ground flour.

Deficiency of lime in the water, and roller-ground flour, present to me a certain fascination as causes of caries and indigestion respectively.

Roller-ground flour has been accused in respect to appendicitis. Portions of steel from the rollers are said to have been found. (Battle, in *Lancet*, 24th August, 1907.)

The modern habit of sterilizing all food by heat, and milk especially, has even been laid under suspicion.

It appears to me that the one among the suggested causes which is found to be most universal, as a habit, for instance, is most likely to be found the true source of the trouble.

Whatever else may be said, this much is true, that here again "civilization is a failure." The civilized have both dental caries and appendicitis, the uncivilized have not.

It is undoubtedly therefore, in some habit of our so-called civilization, that the cause of both diseases is to be found. It is in some custom, hitherto the property of uncivilized races, that the means of prevention will be found.

REASONS FOR BELIEVING APPENDICITIS TO BE ASSOCIATED ETIOLOGICALLY WITH DENTAL CARIES.

1. The undoubted increase in appendicitis has gone *pari passu* with a similar increase in dental caries.

2. Almost every patient with appendicitis has dental caries or deficient molars.

3. Almost every patient has had recent trouble with dental caries, tenderness or pain in molars.

4. Infants suffer rarely.

5. Appendicitis commoner in the younger generation, because they have bad teeth, worse than their elders.

6. Dental caries probably a more frequent cause than want of teeth. Appendicitis less frequent in those whose whole set of teeth is artificial—none of their own.

7. A consideration of bacteria concerned in each.

8. Flatulent dyspepsia especially due to dental caries, and apparently intimately associated with appendicitis.

9. Microbes of dental caries produce fermentation.

10. Of the microbes mentioned as discovered in appendicitis, viz.:—bac., com., coli.; streptococcus; staphylococcus; aureus; tubercle.

11. Black races have good teeth; no appendicitis.

SOME APPLICATIONS OF THE PRECIPITIN REACTION IN THE DIAGNOSIS OF HYDATID DISEASE.

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University of Sydney.)

Results previously obtained.—In January, 1908, two of us* published an account of our experience with the precipitin reaction in hydatid disease, and quoted nine consecutive and unselected cases of hydatid invasion in all of which a well-marked precipitin reaction was obtained. In eight of these cases blood from the hydatid patient was collected in sterile glass pipettes on the morning of the day of operation; the hydatid fluid or cysts were obtained in sterile flasks in the afternoon; the patient's serum and the hydatid fluid were allowed to stand overnight in a cool chamber, and the test was made next morning about 24 hours after the blood had been collected. Control sera from healthy persons were taken at the same time as the sera to be tested, and were kept in the same manner. From six to twelve drops of serum were shaken up with 1 c.c. clear hydatid fluid, precautions to minimize bacterial infection being taken, and the tubes were allowed to stand 24 hours at room temperature before the precipitates were read. In these circumstances, with one exception, the patient's serum never failed to give a positive reaction with the hydatid fluid removed from his tissues. The single exception, however, yielded a precipitate when the patient's serum was tested with the hydatid fluid obtained from another patient. While this was our usual procedure, we also tested the serum of some of the patients, collected before operation, with hydatid fluids taken from other patients, and we obtained positive precipitin reactions in cases in which subsequent operation revealed undoubted hydatid invasion, and negative precipitin reactions in doubtful cases in which at operation hydatid cysts were not found. Notwithstanding our small series of cases, the fact that in every instance the presence of the reaction corresponded with the presence of hydatid cysts, and the absence of the reaction with the absence of cysts, led us to the conclusion that in the precipitin reaction we had a valuable aid to the diagnosis of hydatid disease. We must, however, emphasize the fact that this striking evidence of the correlation of a specific precipitin reaction with hydatid invasion was obtained in the most favorable conditions of experiment, since usually the serum and the hydatid fluid were taken from the same patient and tested within 24 hours, or, when the serum and the hydatid fluid were obtained from different patients, the hydatid fluid had not often been kept for more than a few days.

Object of present paper.—It was obvious, however, that if the test was to be of general value as a means of diagnosis, we must have a method of storing, for considerable periods of time, hydatid fluid in such a form that it would be capable of giving a well-marked reaction with the sera of patients suffering from hydatid disease, and that it would not yield any reaction with sera obtained in other conditions of disease. Our present communication, which is of the nature of a progress report, deals mainly with the interactions of hydatid fluids (a) not derived from the patient

* Welsh and Chapman, *Austral. Med. Gazette and Lancet*, 1908.

whose serum was being tested, (b) of longer standing than those used in our first series of cases, and (c) subjected to various treatment in order to preserve them in a sterile condition for many months. The conditions of experiment were, therefore, less favorable than in our first series of cases, and we may at once state that in this second series, the amount of the precipitum in positive cases was on the whole smaller than in our first series, and that some cases of hydatid invasion failed to give a satisfactory reaction with the hydatid fluids employed by us. Nevertheless, in no single instance did we obtain a positive reaction where hydatid invasion was afterwards proved not to exist, and we were able to give a positive diagnosis in some cases in which the clinical evidence was on the whole opposed to the probability of hydatid disease, our diagnosis being confirmed by subsequent operation.

Data on which paper is based.—Exclusive of control observations, this paper is based on 50 precipitin reactions tested in regard to 36 patients known or suspected to harbour hydatid cysts. Of these patients, 20 were definitely proved to be infested by hydatids, and 30 precipitin tests were carried out in regard to them, in addition to the usual controls. The analysis of these 30 reactions, the nature of the hydatid fluids employed, and the results obtained, constitute the substance of our paper. We may note, however, that of the other precipitin tests, 11 were conducted with the serum of 10 patients, from whom hydatids were in all probability absent, since an operation, or the subsequent history, revealed the presence of some other condition (new-growth, suppuration, calculus, &c.), sufficient to account for the symptoms. In all of these cases, the precipitin reactions were negative. The remaining nine precipitin tests were made with the serum of six patients in whom an operation, or the progress of the case, was inconclusive as regards the presence or absence of hydatid invasion. Of these doubtful cases, two gave a slight positive reaction, and four a negative reaction. Since the diagnosis was doubtful, we have not dealt further with this group of cases.

LIST OF 20 CASES INVESTIGATED, IN WHICH CONCLUSIVE EVIDENCE OF
HYDATID INVASION WAS OBTAINED AT OPERATION, OR OTHERWISE.

Case 1. R., male 23 years; large single cyst of liver with clear contents.

Case 2. F., female, 40 years; multiple suppurating cysts of liver.

Case 3. H., female, 24 years; hydatid of liver.

Case 4. A., male, 39 years; multiple cysts in muscles of back, cysts densely matted and packed with minute secondary cysts.

Case 5. D., female, 12 years; multiple cysts of kidney.

Case 6. T., female, 26 years; large cyst of liver with clear contents.

Case 7. K., male, 26 years; multiple cysts in kidney of at least ten years' duration.

Case 8. L. R., male, 69 years; multiple small cysts in liver.

Case 9. F., male, 39 years; cyst on under surface of liver, compressing portal vein and simulating cirrhosis of liver.

Case 10. B., female, 18 years; large cyst of liver with clear contents.

Case 11. P., male, 31 years; cyst behind liver.

Case 12. M., male, 11 years; suppurating hydatid in left iliac region.

Case 13. L., male, 42 years; large cyst of liver with clear contents.

Case 14. L., female, 50 years; large shrunk, degenerated cyst of liver with caseous, bile-stained contents.

Case 15. W., female, 28 years; medium cyst of liver with clear fluid contents.

Case 16. W., male, 42 years; hydatid of liver.

Case 17. H., male, 35 years; hydatid of lung (?); no operation was performed, but patient coughed up a piece of tissue which we identified as hydatid membrane.

Case 18. W., female, 12 years; cyst of liver with clear fluid contents.

Case 19. R., female, 49 years; multiple cysts of liver, spleen and peritoneum.

Case 20. H., male, 22 years; large cyst of liver with clear contents.

ANALYSIS OF THE PRECIPITIN REACTIONS OBTAINED WITH VARIOUS HYDATID FLUIDS, TESTED WITH THE SERA OF THE PATIENTS IN WHOM THE PRESENCE OF HYDATID CYSTS WAS CONCLUSIVELY PROVED.

The tests may be arranged in the following groups, according to the nature of the hydatid fluid employed in the interactions:—

Group I.—In this group of seven tests, we repeated the methods adopted in our previous investigation of allowing the serum of the patient to interact with the fresh untreated hydatid fluid taken from the cysts harboured by himself, or with similar fluid collected from another patient. In every instance but one, definite evidence of a precipitate was obtained in 24 or 48 hours at room temperature. In two cases the precipitate was very slight, but distinct in comparison with the control tubes; in two cases the precipitate was well marked, and in two it was massive. In one case of long-standing hydatid invasion (case 7), the reaction was indeterminate.

(1) 1 c.c. fresh hydatid fluid from case 4 interacting with nine drops serum from same case, taken just before operation, gave a well-marked precipitate.

(2) 1 c.c. fresh hydatid fluid from case 6 interacting with nine drops serum from same patient, before operation, gave a slight positive reaction.

(3) 1 c.c. hydatid fluid from case 6 passed through Chamberland filter, interacting with sixteen drops serum from same case, seven days after operation, yielded a very large precipitate of 4 mm. depth.

(4) 1 c.c. fresh hydatid fluid from case 7 interacting with twelve drops serum from same case, collected before operation, gave an indeterminate reaction.

(5) 1 c.c. fresh hydatid fluid from case 7 interacting with twelve drops serum, taken before operation from case 8, gave a well-marked reaction.

(6) 1 c.c. hydatid fluid from case 18, collected sterile and kept on ice, interacting with eleven drops serum, taken from same case five days after operation, gave a large precipitate exceeding 2 mm. in depth.

(7) 1 c.c. hydatid fluid from case 18, kept sterile on ice for five days, interacted with eleven drops serum from case 19 before operation, and gave a slight positive reaction.

Group II.—The tests in this group were conducted with the fluid obtained from a large hydatid cyst of the liver in case 1. The clear fluid was passed through a Chamberland filter, became accidentally infected, and was filtered again, thereafter remaining sterile. After the second filtration, and six days after removal from the patient, quantities of the fluid amounting to 3 c.c. and 4 c.c. were put into sterile tubes, without further addiment, the tubes were plugged and allowed to stand at room temperature. It was then noticed that a deposit began slowly to separate

out from the hydatid fluid. For a few days no deposit was seen at the foot of the tubes; by the end of a week a slight trace of deposit was visible; by the end of a month well-marked deposits were present, the fluid remaining quite sterile. With this fluid was tested the serum from each of two hydatid patients (other than the patient from whom the fluid had been obtained), but in each case the result was indeterminate, since the deposit was too scanty to warrant a positive diagnosis being made. Owing to its failure to elicit definite precipitates from the sera of two hydatid patients, this fluid was discarded.

(1) 1 c.c. hydatid fluid from case 1 as above, six days after removal, interacted with twelve drops serum from case 2, collected one month after operation, while small cysts continued to drain away from the wound. A large quantity of serum was obtained, and four similar reactions investigated, tubes being allowed to stand at room temperature for 24 hours, at 37 degrees C. for one hour, at 45 degrees C. for one hour, and at 60 degrees C. for one hour; but in every case the result was indeterminate.

(2) 2 c.c. hydatid fluid from case 1 as above interacted with eight drops serum from case 3 before operation, but again the result was indeterminate.

Group III.—A saline extract of hydatid membrane was used in the third group of tests. The membrane was dried in a dessicator over calcium chloride, and at a temperature of 37 degrees C. Six grammes of dried membrane were ground up in a mortar and extracted for 48 hours with 300 c.c. of a 0.75 per cent. sodium chloride solution. The extract was then passed through a Chamberland filter. With the filtered extract was tested the serum from each of four hydatid patients, but in each case with negative results. It must be added, however, that in one instance (case 5), the serum was not tested until three weeks after operation, and only four drops were obtained; in another instance (case 6), the serum before operation gave only a slight reaction with the fresh hydatid fluid from the same patient; in a third (case 7), the reaction of the serum with the fresh fluid from the same patient was indeterminate; and in a fourth (case 13), the serum was not otherwise tested.

(1) 1 c.c. extract hydatid membrane interacted with four drops serum from case 5, collected three weeks after operation, while cysts were being still discharged, and gave a negative reaction.

(2) 1 c.c. extract interacted with nine drops serum from case 6, before operation, to give a negative reaction.

(3) 1 c.c. extract interacted with twelve drops serum, taken before operation from case 7, with a negative result.

(4) 1 c.c. extract interacted with nine drops serum from case 13, and gave a negative reaction.

Group IV.—The tests in this group were carried out with the hydatid fluid obtained from a liver cyst in case 6. The clear fluid was passed through a Chamberland filter, and thereafter remained sterile. It was stored in small flasks, and, after the lapse of a month, it was noticed that an abundant flocculent deposit had separated out in all the flasks, the superfluid remaining clear and sterile. This fluid had been tested with serum from the same patient one week after operation, and gave a very large precipitate (*cf.* group I, No. 3). It was then used for purposes of diagnosis, to test the serum taken before operation from seven different hydatid patients, and gave distinct positive reactions with four of these, and indeterminate reactions with the remaining three, in one of which the only cyst found was shrunken and degenerated. In two of the positive cases the circumstances were of more than usual interest. In case 9 two

separate precipitin tests were made in each instance with definitely positive results. Nevertheless, operation was delayed because the clinical signs pointed so strongly to cirrhosis of the liver. Later on, however, an operation was performed, and a hydatid cyst was found on the under surface of the liver, compressing the portal vein. Again, in case 11, a positive reaction was obtained, but at the first operation no lesion was found, and the wound was closed up. A second precipitin test gave again a positive reaction, and a second operation was performed, when a hydatid cyst behind the liver was found.

(1) 1 c.c. fluid from case 6, one week old, interacted with twelve drops serum from case 7, before operation, and gave a slight positive reaction. The serum from this case (7) gave a negative reaction with extract of hydatid membrane (*cf.* group III, No. 3), and an indeterminate reaction with the hydatid fluid from the same patient (*cf.* group I, No. 4).

(2) 1 c.c. hydatid fluid from case 6, one month old, interacted with ten drops serum taken before operation from case 9, and gave a decided positive reaction. Operation was delayed (*v. supra*), and the test was repeated with similar results.

(3) 1 c.c. hydatid fluid from case 6, two months old, interacted with thirteen drops serum taken before operation from case 10, but only a very slight precipitate was obtained, and the result was indeterminate. A few days later the test was repeated, but the result was still indeterminate.

(4) 1 c.c. hydatid fluid from case 6, two months old, interacted with eight drops serum from case 11, before operation, and gave a slight positive reaction. At the first operation, no lesion was found, and the test was repeated with again a slight positive reaction. A second operation revealed the presence of a hydatid cyst (*v. supra*).

(5) 1 c.c. hydatid fluid from case 6, two months old, interacted with thirteen drops serum taken before operation from case 12, to give a slight positive reaction.

(6) 1 c.c. hydatid fluid from case 6, four months old, interacted with eleven drops serum taken before operation from case 14, but the precipitate was too slight to justify a positive diagnosis. At operation only a shrunken degenerated cyst was found.

(7) 1 c.c. hydatid fluid from case 6, four months old, interacted with fourteen drops serum from case 15, before operation, and gave an indeterminate result.

Group V.—Our fifth group of precipitin tests includes those conducted with clear hydatid fluid obtained from the liver cyst in case 15, passed through a Chamberland filter, and treated with sodium arsenite, 50 c.c. of the filtrate receiving 0.5 gramme sodium arsenite. With this fluid three tests were made on hydatid patients before operation, in two instances with positive results, and in one instance with a negative result.

(1) 1 c.c. hydatid fluid from case 15, + sod. arsenite interacted with eight drops serum taken before operation from case 16, and gave a slight positive reaction.

(2) 1 c.c. hydatid fluid from case 15, + sod. arsenite, interacted with twelve drops serum from case 17 before operation, and gave a slight positive reaction.

(3) 1 c.c. hydatid fluid from case 15, + sod. arsenite, interacted with fourteen drops serum from case 20 before operation, but no precipitate was seen.

Discussion of Results.—While we have obtained further evidence of recognisable and specific interactions between fluids from hydatid cysts

and sera from hydatid patients, we have not been able to find a method of applying this reaction so that it will hold good in all cases in conditions of diagnosis. This failure appears to be due to difficulties relating to the serum of hydatid patients and to difficulties relating to the hydatid fluids. In regard to the serum (antiserum) we must emphasize the great differences that exist between the hydatid antiserum arising in the human subject in conditions of accidental infection and precipitin antisera prepared by artificial injection of animals. In preparing a rabbit, in order to obtain a precipitin antiserum that will interact with blood serum or with egg-white to yield a good precipitate, moderate doses of the protein (from 0.5 to 1.0 gm. dried protein) are introduced at intervals of two or three days, until from six to ten such doses have been given, and the antiserum is withdrawn ten or fourteen days after the last injection. The precipitable content of such an artificial antiserum will be diminished, or may even disappear, if the injections are inadequate, excessive, irregular, or prolonged, or if the interval between the last injection and the withdrawal of the antiserum is too short or too long. With these somewhat rigid conditions, essential to the preparation of a good precipitin antiserum, we must contrast the conditions of protein interchange when the human subject becomes invaded by hydatid cysts. In the first place, absorption of material from the hydatid cyst extends usually over a long period of time—certainly for months, often for many years—before the patient comes under observation, and the capacity of the human organism for reaction to the hydatid products may thus become exhausted. Moreover, the absorption of hydatid products may often be inadequate, and more often still it is probably irregular both in amount and in time. On these grounds alone, it is only reasonable to expect that the precipitable content of hydatid antisera from the human subject should inevitably be small in the majority of cases, and it is not surprising that in some cases it should be inappreciable.

In regard to the hydatid fluid, we must note (*a*) that not all hydatid fluids are equally capable of interacting with hydatid antisera to give a precipitate, and that some appear to fail altogether; (*b*) that, when hydatid fluid, filtered or unfiltered, is allowed to stand in a sterile condition, a deposit slowly separates out and may form a considerable layer, and that this spontaneous precipitation may be associated with deterioration of the capacity of the fluid for interaction with hydatid antisera; (*c*) that the low saline content of hydatid fluids may interfere with the precipitin reaction, since precipitin interactions are very susceptible to variations in the saline content of the fluid in which the interaction takes place. There may, however, be some way of overcoming the apparent unsuitability of some hydatid fluids for reaction with hydatid sera, for example, by modification of their saline content, but this last point did not occur to us while carrying out these tests.

Conclusions.—(1) A precipitate is not obtained in any interaction between hydatid fluid and human serum from a patient not invaded by hydatid cysts. (2) Having regard to the long duration and probable irregularity of the interchange of protein, molecules between a hydatid cyst and its human host, and having regard also to the possible unsuitability of the hydatid fluid, we must expect a certain number of precipitin tests to fail to give a satisfactory precipitate, and this has been our experience. (3) In the conditions of a diagnostic precipitin test, as above described for patients suspected to harbour hydatid cysts, a negative reaction is inconclusive, but a positive reaction is conclusive of hydatid invasion, and the latter may be obtained in circumstances of clinical importance.

THE PREPARATION AND AFTER-TREATMENT OF PATIENTS SUBJECTED TO OPERATION.

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The preparation of a patient prior to undergoing an operation is, to my mind, one of the most important matters connected with operations generally. It has certainly not received the attention that it deserves; in the past and at the present moment it is very far from being satisfactory.

In all cases then about to be submitted to operation, where time permits, a few days should elapse before the actual performance of any operation, and it is during this period that investigations should be made which will prove of great help to the surgeon. The regulation of the diet and the administration of aperients naturally commend themselves in all cases. The mere giving of aperients might seem a simple matter, but I think it is generally admitted that calomel, followed by salines, are the most appropriate of all aperients in operative cases. Every patient, then, during this period of inactivity should be examined with a certain definite object in view—with the idea of ascertaining how certain natural processes are being carried on. By these means we can estimate with fair accuracy the capacity of any patient to stand the additional strain of an operation. The scope of inquiry may be placed in a certain definite order, but, of course, any order will do so long as it is fully carried out. (1) The temperature. (2) The state of the pulse and the blood-pressure generally. (3) The estimation of the volume-index and the colour-index of the blood, followed by a differential leucocyte count. (4) Examination of the urine, and in all cases an estimation of the amount of urea passed in the 24 hours. (5) I am not forgetting the general examination of the patient or the information it may have afforded. What I have put down may be designated the minimal requirements in all proposed cases of operation, and, consequently, none of which may be omitted. For purposes of a short consideration, these simple investigations may be taken in the order in which they stand. The temperature is of much value when systematically and accurately taken. A small matter, but of some importance, is that it should be taken in the mouth, under the tongue, and, for the purpose under review, at least four—hourly. In most instances, the presence of an increased temperature will be accompanied by other indications, but that is quite another matter. The routine taking of the temperature in patients prior to operation is often the means of directing attention to obscure and unsuspected lesions. The limits of a short paper do not allow one to give specific instances. Whilst the rate, rhythm, and tension of the pulse will be noted as a matter of routine, but will not afford much striking information, it is quite otherwise with the information that careful blood-pressure records are capable of affording. The introduction of blood-pressure readings in the pre-operative procedures that I have assigned them should never be omitted, especially in elderly patients. The information obtained is not only of conspicuous benefit, but is the best method we possess of clearly indicating the condition of the circulation. There are two classes of cases in which its benefits are immediately helpful—arterio-sclerosis associated with cardiac changes, and the same condition associated with chronic nephritis, and of which it may be the product. The gravity of such cases about to be submitted to operation is largely

lessened by the measures that can be at once taken to reduce the blood-pressure for a time at least. In young adults the readings would be, on an average 100 to 135 mm., and about middle life, 120 to 150 mm. What with the multiplicity of instruments and the difference in the width of the armlet, the readings vary a good deal, but not so much as to vitiate their general accuracy. Any reading approaching 200 mm. on any instrument would be very high, and, as a contrast, 70 to 80 mm. would be very low. The Riva-Rocci sphygmomanometer, as modified by Professor Martin, late of the Melbourne University, is a suitable and reliable instrument since the bicycle-pump has been supplanted by a suitable air-bag. The disappearance of the pulse is generally taken as the exact moment when to note the reading, but it is certainly more exact to take the return of the pulse after it has been stopped. The systolic pressure is quite sufficient for all practical purposes. It is scarcely necessary to add that the readings should always be taken when the patient is lying down, and that the arm-piece should be as nearly as possible on a level with the heart. With the hæmocytometers now in use, such as Thoma-Zeiss and others, the volume-index of the blood is quickly and accurately made. With more facility, even the colour-index can be noted with Haldane's hæmoglobino-meter, where gas is to be had, and with Gower's when it is not. For accuracy, the former should always be used, however. The relationship of these two indices is always interesting, even in health, and of incomparable value in certain diseases. If taken alone, the colour-index is certainly the most valuable. In all secondary anæmias the percentage of red corpuscles and the percentage of hæmoglobin are, of course, equal. In chlorotic anæmia, the percentage of hæmoglobin is just half that of the percentage of the red-blood corpuscles. The value of this fact will be appreciated when it is remembered that practically all the anæmias with which we have to do belong to this type; all the various kinds of toxæmias, for instance, tuberculous conditions and the cachexias accompanying malignant disease. This is a wide list, which it is neither necessary nor desirable within the limits of a short paper to particularize. A differential leucocyte count can be made with equal facility as the above, but opens a wide field for speculation more than for exact findings. But, in connexion with the disease of the appendix and the presence or absence of suppuration generally, it has been found of conspicuous benefit. Outside of these two conditions it becomes bewildering in its suggestiveness. That there is a general leucocytosis in many surgical affections is undoubted, and the mere fact that such is the case may be helpful, but nothing more. It is otherwise, however, if we follow the teachings of Gibson (*Annals of Surgery*, 1906). That is—"The relative disproportion of the polynuclear percentage to the total leucocytosis." This, if applied to the lesions of appendicitis and their sequelæ, will be found of real practical help. In watching such cases and debating when to operate, and even when preparing for actual operation, this disproportion, extreme or otherwise, is almost the only means at our command in deciding, in some cases at least, the presence of suppuration or of gangrene in acute affections of the appendix. The mere fact of a general leucocytosis being present is common to so many surgical affections that it is of little or no help. But the disproportion of the polynuclear neutrophiles to the general leucocytosis is so easy to ascertain, and its application so simple, as to constitute the procedure one of real value. I see a good few cases of appendicitis, and every case has to be studied on its merits. Hence any

step carrying with it the above degree of precision is to be eagerly welcomed. The examination of the urine in the matter under consideration is of supreme importance. Urinary findings and their correct interpretation have a distinct and immediate value in pre-operative investigations. Not only to the operator, but to the anæsthetist. When it is remembered that in all cases of anæsthesia by chloroform or ether at least 25 per cent. of the patients suffer from albumen and casts in the urine, the importance of the matter becomes at once apparent. The interesting and practical question at once arises—what anæsthetic should be administered in patients suffering from some form of renal disease. At present, the opinion seems to be held that in such cases ether ought to be avoided, and that the administration of chloroform is much safer. The routine examination of the urine, then, in order to determine the presence of renal disease, is a procedure that we cannot afford to disregard. An examination for the presence of glucose naturally follows the examination for albumen, and it is needless to emphasize the significance of its importance, whether as a mere temporary alimentary or digestive glycosuria or a more permanent and graver condition. The measures which such a knowledge would allow us to take for the safety of a patient are too well known to require comment. It follows, as a natural sequence to the above examination, that the amount of nitrogenous waste excreted by the kidneys in the 24 hours should be ascertained. Fortunately, with the ureameters now in use, this can be quickly and very efficiently done. The standard taken is generally 500 grains in that period, and a few observations gives one the most valuable information, the practical bearing of which on an operation can scarcely be over estimated. It need not be pointed out that the mere presence in the urine of albumen or sugar without some correct idea of the work the kidneys were capable of accomplishing would deprive the whole examination of much of its practicable value. It would add greatly to our knowledge and to the benefit of our patients if one was in a position to state under what circumstances of lowered urea secretion an operation would be considered risky. Mikulicz considered that when the percentage of hæmoglobin in the blood had reached 30 per cent. that it was inadvisable to operate under the usual conditions. I know of no standard, however, in connexion with the excretion of urea that may be taken as a guide.

Under the name of chromocystoscopy, efforts have been made to gain an idea of the functioning power of the kidneys generally. By swallowing, or intramuscular injection, drugs of standard solutions of methylene-blue or indigo-carmin the urine is stained in a certain time, generally in 30 minutes. As one writer remarks—"We have no sliding scale that would enable us to draw reliable conclusions as to the time the stain appears in the urine, or from the intensity of the colouring." However, the very simplicity of procedure makes its trial easy at all times. Cryoscopy, by determining the respective freezing points of the blood and urine, promises fairly accurate results. But it is still more a laboratory than a clinical acquisition, and, as such, is not generally available. The simple methods already mentioned are quite ample for all practical purposes. The actual preparation of a patient who is to be submitted to operation is one to which great importance is very justly attached. Some time since I contributed an article to the pages of *Una* on this subject, addressed exclusively to nurses. In the next issue, an eminent gynæcologist of this town took exception to some of my statements, and, in doing so, made known the fact that he himself was in the habit of preparing

the patients when put on the operating-table. The difficulty of carrying this out where much operating is done appears to me almost insuperable, and the various objections that could be urged against such a course readily occur to the mind, and need not be enumerated. Our knowledge of applied bacteriology in regard to this very matter requires that it be carried out with the most scrupulous care and attention.

The multitude of plans in the preparation of patients prior to operation that have been recommended, and are being continually recommended, show at once that we have not yet succeeded in formulating any one definite plan. It is quite true that the same object can be attained by the most diverse methods; all the same, a want of uniformity is to be deplored. It would not matter so much if the matter itself were in the hands of the operator, but it is just one of those things that he must trust to the care of others. A careful preparation, as we all have good reason to know, is an immense factor in avoiding post-operative complication small enough to cause discomfort only, or great enough to imperil the patient's life. There is no need to mention the fact that we cannot by any means make a skin bacteriologically clean, but, short of this, we may accomplish a great deal. The baths which every patient receives before operation, when not contra-indicated, require no special mention. In the preparation of the actual part to be operated on I attach most importance to careful mechanical cleansing. This is best accomplished with carbolic soap and hot water, so as to produce a thick lather over all the part. It is then covered with gauze and bandaged, the soap poultice being allowed to remain on for three or four hours at least. When this has been accomplished, the part is gone over again with hot water and soap by means of a mop, or, better still, by the use of rubber sponges. By this means, every particle of useless epidermis will have been removed, and the part is ready for the next application. If the skin will permit it, nothing is superior to turpentine. When the part has been carefully gone over with this by using sterile swabs, rubber gloves must be donned for the next steps, or the hands carefully prepared. Absolute alcohol is now used, and followed by 1 to 40 carbolic-acid solution. The part is finally covered with layers of gauze wrung out of the same solution of the strength of 1 in 60. This is covered with waterproof tissue, and a bandage applied. This completes the preparation till the patient is on the operating table, when the part is finally gone over 1 to 3,000 biniodide of mercury solution. The one predominant idea in all preparations, wherever the part may be, should aim at a thorough maceration of the skin, as without this the succeeding steps cannot possibly be effectually carried out. I would also plead for a much wider area of preparation than is usually the case. Take a preparation of the breast, for example. This should include, not only the part to be operated upon, but also the arm and the whole of the chest as well. The few points noted in connexion with pre-operative investigations and preparations can all be carried out in a short time, but I cannot help thinking that too little time is given to hospital patients prior to operation. In the weighty words of Sir Frederick Treves—"In hospital practice it is better not to operate on a man who comes straight to the wards from some active outdoor work, who is robust, and who has been living heartily, and who has the vigorous throb of exercise in his blood and limbs . . . such a patient is placed in an infinitely better condition by a few days rest in a hospital ward. He here becomes accustomed to his surroundings; he has time to be rid of the refuse matter in his tissues, which can no longer be cast off by muscular exertion; his

appetite is enabled to adapt itself to his present requirements; the excreta can be dealt with; and time is allowed to make clean the skin." What is worth doing, he states—"is worth doing well, and not a few operations. the performance of and recovery from, which have to be compressed within a few hurried days, would better not be performed at all." Weighty words, and well worth consideration. Post-operative treatment is such a wide subject that I propose to restrict my few remarks to conditions which may be met with practically after all operations. The choice of an anæsthetic scarcely comes within the scope of my remarks, but it is undoubted that a great deal depends on the selection in certain cases, and that, combined with the time that a patient is under its influence, may have a very material influence on the patient after. It is a mistake to suppose that the doing of an operation as quickly as circumstances permit is abrogated or even modified because the patient is insensible to pain. No patient should be under the influence of an anæsthetic a moment longer than is necessary. To keep a patient deeply unconscious to the end of an operation that has extended perhaps to two hours, till the last stitch is inserted, is a grave mistake. Recent experience and investigation, on lines formerly unthought of, very plainly indicate the dangers of prolonged anæsthesia. The most important matter to engage one's attention after an operation, and the patient has been put to bed, is the question of shock. It may be quite apparent at a glance that none is present. But, after prolonged operations and certain abdominal ones, the question will require immediate and serious attention. It is a condition that operating surgeons are continually meeting with, and no apology need be offered for considering it in some detail. In large hospitals and where clinical teaching is carried out, and, consequently, plenty of assistance available, it would be an excellent plan to have the blood-pressure taken during the whole of an operation, say, every ten minutes. Such assistance to an operator would be simply incalculable, as it would keep him continually informed of the state of the blood-pressure.

It is very necessary in considering the question of shock to bear in mind what the term really indicates, and, thanks to the careful investigations of Crile, in America, and Mummery, in England, a far better understanding of its significance is permitted. The essential thing to remember is that it is a fall in the general blood-pressure; it is constantly to be borne in mind how this is brought about and some of the chief factors concerned in its production. We are also indebted to the same authorities for greatly simplifying our views generally, which has a very direct and valuable bearing on the measures we adopt in treating the condition.

It is now well known that all intra-abdominal operations in the upper abdomen are peculiarly liable to be followed by shock, and for that matter, though in a lesser degree, any abdominal operation. The nerve-centre exhaustion, caused by interference with the afferent nerve paths, is common to the whole body, but in varying degrees, and is also influenced by various forms of injury. In regard to degree, an ordinary afferent path as compared with that of the spermatic cord, and in regard to variety of injury, a crush of a limb compared with its amputation. The essential factor itself is a fall in the general blood-pressure, an excessive collection of blood in the great splanchnic area, due to vaso-motor exhaustion. The condition is exactly the same as that noted, long ago, by Goltz in his experiment on frogs, and many conclusions of a purely empirical kind pointing to the same condition have now received the most ample and precise confirmation. Shock of any kind may be included in this definition,

even that due to loss of blood exclusively, and to which I will refer when speaking of treatment. According to Crile, the falling blood-pressure is compensated by the vaso-motor mechanism, and if the depression is not too great this may amply suffice. If it does not, then the helpful action of the heart comes into play. Savory, whose writings for a long period were regarded as authoritative on the question of shock, states—"The heart is powerfully affected through the nervous system, and its action is arrested." Such is by no means the case. If the vaso-motor mechanism fails to re-establish the falling circulation, the aid of the heart comes into play in a manner it is necessary to clearly understand. Through its sympathetic, augmentor, or accelerator mechanism, centres, and nerves, it is now called into action, and a decided increase in rate is the result, and is a most important factor in keeping the blood-pressure at a sufficient level. If this assistance fails, a condition is established which very quickly leads to a dangerous condition of the circulation generally. The blood will now rapidly tend to accumulate in the large venous trunks of the splanchnic area, with its inevitable corresponding decrease in the arteries. All the chief centres now begin to suffer from a deficient blood supply, and this is intensified by impaired aëration. When the depleted vessels have exhausted the supply that they draw from the tissues, another most disturbing factor is introduced in the increased specific gravity of the blood. The vicious circle is now fully established, and with a knowledge of the various events leading up to it we are in a position to take measures for its restoration to a state compatible with life. I am not forgetting that the condition here described may vary in intensity within wide bounds, but as these are clinically easy of recognition, we will not be led into under-estimating any case that demands prompt attention.

At the present moment the condition of the patient in regard to shock is only attended to at the conclusion of the operation, or when put to bed. I hold that, by having the record of the blood-pressure taken whilst the patient is actually undergoing the procedure inducing it, is the only way to put us in a reliable and strong position in regard to treatment. By the time that shock is fully established, the most precious time for its treatment has been missed. This is a matter that admits of no denial, and such being the case, surely the time is not far distant when the progress of shock from its very inception can be watched, its amount noted, and effectual measures taken long before it has reached an alarming stage.

From the researches of Mummery in one of the largest London hospitals, the percentage of death from shock is about 21 per cent. That 21 patients of every hundred operations should die from a condition that is largely preventable is a fact that ought to arrest the serious attention of operating surgeons. The measures taken at present for combating a moderate degree of shock are well known and easily applied. Although of a very elementary character, it is necessary to allude to them in order to get a complete idea of the treatment generally. The diminished quantity of blood available renders it imperatively necessary that as much as possible should be sent to the large nerve centres, on whose integrity the circulation depends. The limbs are at once enveloped in cotton wool and bandaged, the same being done with the abdomen, external heat at the same time being promptly and variously applied. A modified Trendelenburg position then follows by raising the foot of the bed to any required distance. These measures will find an intelligent interpretation when it is remembered that at this time the heart may be taxed to its utmost to help the vaso-motor mechanism.

and taxed, be it remembered, because it has insufficient blood to work with, that the blood is largely confined to the splanchnic area, and that its consistence has been largely increased. As the heart depends for its intake on the pressure in the vena cava, and not on the aortic blood-pressure, the reason of this posture will be appreciated. The next consideration is how to increase the volume of the circulating fluid, as by this time all available fluid will have been drained from the tissues. When the blood-pressure is not too low and a moderate amount of absorbing power is present, a pint to a pint and a half (repeated) of hot saline is given, with the usual precautions, per rectum. To this is now generally added in all cases a suitable dose of solution of adrenalin chloride. The former will help in filling the depleted arteries, and the latter is invaluable in toning up the peripheral circulation. It has been found, experimentally at least, that this is the best preparation of all the various kinds of vasoconstrictors for the above purpose. There are three very valuable measures accomplished by this simple procedure—increased quantity of fluid in the vessels, a dilution of the already viscid blood, and increased tension in the peripheral circulation. The immediate effects of this are too obvious to need comment. I would add to these simple measures the hypodermic administration of standardized solution of aseptic ergot, such as is now easily obtained in suitable ampoules. This is a remedy of the highest value, and its immediate effects on in raising the peripheral resistance is immediate and striking. A remedy, or a supposed one rather, that has long held its place in this connexion is strychnine.

The injection of strychnine has been, in the past, based on an entirely erroneous conception of the state of the circulation present in these cases. Its use is pernicious except, and that only occasionally to support the respiratory centre. Such measures as here indicated may suffice to bring the circulation to a safe level, if not, the intra-venous transfusion of saline fluid is imperative. This is a measure of the highest importance and of the utmost value, and in cases of shock from hæmorrhage is nearly always efficient. That being so, it is a matter for regret that so many different ideas prevail regarding it, or that it is made use of by different procedures. The matter is so simple that it ought to be easy to have uniformity of practice. May I here be permitted to allude to what appears a small matter, but which has really some importance—that the salt used should always be ready in tablet form to put into the hot sterile water. I have seen lamentable consequences result from the hurried use of ordinary salt in matters of urgency such as this. Tablets are now so easily obtained that there is no excuse for not having them on hand. The heat of the fluid should be carefully tested, and should be about 110 F. The amount should be 2 pints, not more, perhaps a little less, and should always have added to it some adrenalin solution, say 10 minims of the 1 to 1,000 adrenalin chloride solution. This should be introduced into the vein by means of a sharp hollow needle, a little larger than an ordinary hypodermic one, and attached to the tubing of one of the many transfusion apparatus that can now be obtained. It is always best to have one fitted with a thermometer, so that the heat of the fluid can be seen continuously. About a quarter of an hour should be consumed in introducing the above amount, a pause being made at any time when the breathing becomes embarrassed. This is not common with fairly slow administration. When finished, the needle is detached from the tubing and left in the vein. The external end may be plugged with a little sterile gauze, and as much of the needle as projects be surrounded with

the same material, the whole being carefully covered with a bandage. In cases of any gravity, it is not good practice to try and dissect out a vein at the bend of the elbow. Valuable time may be lost in doing so, and often great difficulty will be experienced. By using a sharp needle, and pinching up a fold of skin at the elbow, the introduction is easily effected. When a few drops come through, it will be seen at a glance whether the needle is in the vein or not. The limit of 2 pints of fluid, and the leaving in of the needle, I regard as indispensable. There is no doubt but that with an excess of fluid the tissues generally may become water-logged, and what is of more consequence, there is always the danger of pulmonary oedema to be reckoned with. An excess has also a marked and deleterious effect on the red-blood corpuscles. There is not the least fear of any clotting in the retained needle, and it may be used again and again if circumstances demand it. Ergot may be administered, when required, to brace up the peripheral circulation. Alcohol and any other drugs at this stage are only harmful. As soon as the heart is fairly started, however, with enough fluid for its need, the question can be considered from quite another stand-point. The administration of $1/100$ of a grain of digitalin, and the same quantity of sulphate of atropine, will at this stage often prove of much help. Should the blood-pressure again show signs of falling, more fluid can, again and again, if needed, be poured into the circulation, generally with the happiest results. The treatment of shock due to hæmorrhage is usually quite successful if the condition is recognised sufficiently early. I have never been able to see the distinction drawn between ordinary shock and that the result of hæmorrhage, so far as treatment is concerned. In the one case, a patient is bled into his own vessels; and, in the other, it escapes from them; but the resulting influence on the circulation is just the same. So, for the purpose of treatment, it is as well to regard their effects as identical, and in this way there will be less confusion and more uniformity in treatment. I am not forgetting in this connexion the loss of the oxygen-carrying corpuscles in the one case, and their preservation on the other, but this has to do with prognosis, not treatment. The introduction of large quantities of fluid into the tissues, or hypodermoclysis, as it is called, is a procedure to my mind deservedly falling into disuse. I have noted three cases where this was carried out in two cases of shock and one of hæmorrhage, by others. Though capably and carefully done the result was disastrous. If it is borne in mind that the circulation, or rather the general blood-pressure, when this is done is at a low level, it is not difficult to foresee what may happen. A large quantity of fluid is introduced under a certain pressure, under conditions where there may be practically no absorption, and the small peripheral vessels empty. Gangrene of the part is easily produced, and another grave factor is added to a condition that could scarcely be worse, and little, if any, compensating advantage received. If the fluid promptly entered the vessels and immediately raised the blood-pressure, some drawbacks might be put up with, but even that is not in many cases attained. With the means at our command then, and a better understanding of the conditions underlying the state of shock, I see no reason why its high rate of mortality should be maintained. If the erroneous ideas regarding the heart in shock were cleared away it would very materially help us. The heart has practically nothing to do with shock in its early stages, and there is only one condition in which it has, and that is the shock produced by an overdose of chloroform. May I be permitted to say again, as the matter so vitally concerns treatment, that the apparently weak heart

as shock progresses is only an accelerated heart endeavouring with an inadequate supply of fluid, to support the vaso-motor mechanism and raise the blood pressure. The venous pressure has fallen so low as to at once interfere with the heart's intake, and hence it becomes secondarily affected. If the primary vaso-motor breakdown is severe enough, recovery is scarcely possible. All the more reason then that the first indication of its going wrong should be made known to a surgeon during the operation, by a continuous blood-pressure record.

We have long been under the impression that after a patient's recovery from an anæsthetic, and when the more immediate effects had worn off, that the matter was done with. There may have been slight albuminuria, with casts, as there is in about 25 per cent. of cases anæsthetised, but that would likely pass off favorably. There might also be some lung trouble of a temporary character if either had been used for a long period. But our responsibilities in post-operative work have been materially increased since it has been recognised that a grave condition may follow the long administration of chloroform even when it is given for a moderate period.

Acidosis, or delayed chloroform poisoning, is a post-operative condition that we can no longer disregard. Acidosis is not confined to the administration of chloroform or ether, but may be found in cases of diabetes, in conjunction with fever, prolonged hunger, a salt-free diet, and in cases of carcinoma. It is its association with the administration of anæsthetics, however, that chiefly concerns us. It has now assumed sufficient importance to warrant an examination of the urine, either before or after operation, for the indication of substances that would herald its probable occurrence. The tests are so simple and easily applied, that the examination is a comparatively simple matter. "Of all the organic acids B-oxybutyric is the most important in this respect, for it appears in the urine not so very infrequently, and is sometimes excreted in the urine in enormous quantities. If this acid be oxidized, it is transformed into diacetic acid, and later into aceton, so that these three bodies are very clearly related, and they have been collectively termed the aceton bodies, and all three are excreted by the kidneys. Normally these aceton bodies are oxidized into carbon dioxide and water in the body, and only traces, at most, of aceton are normally excreted by the urine."—(Krehl Clinical Pathology). There is a natural tendency in the body to increased formation of ammonia, during the formation of these products which has a neutralizing effect. The method of treatment is the introduction into the blood of saline solutions in a more or less concentrated state, preferably bicarbonate of potash. No doubt this treatment might be effectual if adopted at an early stage, hence the necessity of an examination of the urine prior to and after an operation. In a case of acidosis, published by Dr. McArthur, in the *Intercolonial Medical Journal of Australasia*, for August, 1907, a reference is made to the symptoms present when acid intoxication has developed. The first symptom noted was a constant desire to yawn, and, curiously enough, the same symptom is alluded to in a case by Mr. G. A. Syme in the same *Journal* for November of the same year. Strange to say this was noted in a case of my own. The symptoms are extremely vague and indefinite, and may develop in a way that will entirely put us off our guard, without we are in constant touch with the findings of the patient's urine. It would be quite impossible here to define them, except a few leading ones, but any one of which standing alone would have no particular significance. For instance, a longer continuance

of the vomiting, than what the influence of the anæsthetic or the onset of sepsis would account for, a restless condition generally, a constant desire to get out of bed, when rest in bed is imperative, a disinclination to take food or medicine, talking with slight incoherence, coated tongue, a thirst which is almost abnormal, a faint icteric tinge of the conjunctivæ, the sleep deeper than natural, and the breathing slightly stertorous, a constant restlessness seen in arranging and disarranging the bedclothes. Without warning the patient will perhaps become comatose, and die. As before stated, the interpretation of these symptoms is hopeless without the aid of a constant urinary examination. Unfortunately, the time at my disposal will not allow me to touch on many post-operative conditions, such as diet and posture in abdominal operation, and also the administration of aperients. Might I conclude by stating, in regard to post-operative vomiting from the effects of the anæsthetic, that, in my opinion, the patients generally are too much restricted in regard to fluids, and that a routine-washing out of the stomach is both desirable and necessary.

RENAL CALCULUS.

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Before dealing with the operative aspect of this subject, I desire to draw your attention to two auxiliary, but essential aids, to diagnosis. These are cystoscopy and X-rays. As regards cystoscopy, I consider that every case of renal calculus—indeed all renal cases—should be submitted to cystoscopy before operation. This has been the practice at St. Peter's Hospital for stone for some time past. Not that cystoscopy will tell whether there be a stone in the kidney or not; but it will give us useful information, and in some few cases may be the means of averting a mistake—as in cases which have been reported, where a papilloma over the ureteric orifice has been solely responsible for renal symptoms. We can see whether there are two ureteric orifices, their condition, and the nature of the efflux, and thus get some clue to the presence of a second kidney, and its condition. Also, a small stone, which may have been revealed by X-rays to be in the kidney, may have passed into the bladder. Hence the examination should be made only a short time before operation. Again, if the condition of the other kidney to the one to be operated on is doubtful, and some estimate of its functional state is desired, I consider the best method to pursue is that now being practised and recommended, especially by Koprammer of Vienna. This is to inject methylene blue intra-muscularly, and then with the cystoscope watch the ureteric effluxes, noting the time the blue appears, and the time that elapses before its maximum intensity is reached. This is not so accurate as the results obtained by ureteric catheterization, but efficient practically; and I do not consider that this latter (catheterization) ought to be practised except on a few unusual cases. It is too dangerous to the patient to be used as a routine method.

With regard to the X-rays. A paper of great excellence has already been read in another section of this Congress on this subject, and I was

glad to find that the reader laid emphasis on the points I consider most important. Briefly enumerated, these are :—

- (1). The radiographer should, if possible, be a medical man.
- (2). Both kidney regions should be radiogrammed, and, where possible, the whole of the urinary system.
- (3). A plate must be taken. Screening a case is worse than useless.
- (4). The plate must be read. A print is of little help. It is best for the surgeon to interpret the plate for himself.

With good work every stone will be seen, whatever the nature, and in most cases the outline of the kidney as well. An especially shielded light is of great assistance in reading plates.

In discussing operative treatment the first question which requires consideration is what incision is to be used. It is never necessary to open the peritoneal cavity in attacking a calcareous kidney. The incision which is commonly used is the so-called ilio-lumbar, running parallel to the iliac crest in the direction of the fibres of the external oblique. The advantage claimed for it is that, if necessary, it can be continued downwards so as to expose nearly the whole length of the ureter, when that tube requires exploration. I do not consider, with improved methods of diagnosis, that this should be necessary. In cases where there is a stone in the ureter this incision is the best. In uncomplicated kidney cases, however, I think the vertical incision, outside the latissimus dorsi, recommended by Edebohls for use in nephrorraphy is most suitable. If sufficient room is not obtained by this, or in cases where the iliac crest and the last rib are closely approximated, the lower end of this incision can be extended in a downward and forward direction, with good result. The ideal in any incision is that it should do the least possible damage with the greatest possible exposure of the part required. This vertical incision answers these requirements well—indeed the kidney can often be well exposed without the division of a single muscle fibre. It is well to use Edebohls' air cushion. This is much more satisfactory than the old-fashioned sand-bag. Before anything further is done all adhesions must be broken down, and the organ brought well into the wound—if possible on to the loin. In a few cases this may not be possible. Then a head light is essential. No time should be wasted in attempts to feel the stone through the kidney substance. In all cases where the diagnosis warrants operation the kidney must be opened, and a surgeon is quite unjustified in saying that no calculus is present because he cannot feel it when the kidney is exposed. The kidney must be opened in every case. The large majority of stones are in the pelvis, usually adherent to the mucous membrane, as Hurry Fenwick teaches. These have generally been localized beforehand by X-ray. The procedure I recommend is to tilt the organ forwards and expose the posterior surface of the pelvis. Open this by a vertical incision, and on the introduction of the finger the stone can usually be located. If this is not sufficient the incision should be extended straight upwards through the kidney substance as far as necessary. It can be carried almost to the convex or outer border of the organ, and by this means the whole internal aspect of the pelvis can be exposed to view. It is seldom necessary, however, to make so large an opening. Now, I am aware that the ordinary method adopted is an incision through the convex border along the white line, and Continental surgeons often hemisect the kidney in this direction. Now, with this incision there is very free and troublesome

hæmorrhage—in the pelvic incision there is practically no bleeding. Again, the deep sutures necessary to close the cortical incision are placed transverse to the blood vessels, so that these are occluded and the blood supply cut off. Indeed, though I have not been able to examine a kidney some years after this incision has been made, I think that a large area of useful secreting kidney substance is always destroyed by this procedure. The same objection does not hold against the vertical extension of the pelvic incision.

If the stone is not found at first, it may be necessary to make small incisions into either pole—then justifiable, for no corner must be left unexplored. Cases have often been explored, nothing found, and the diagnosis blamed. In many of these I feel confident that a stone was present, and would have been revealed if sufficient search had been undertaken. I have seen several such cases myself.

The removal of the stone may be difficult, but can usually be accomplished with a pair of dissecting forceps and a small lithotomy scoop.

The incision must then be carefully sutured—the suture being made to control any hæmorrhage. Catgut is the material for choice. It is quite unjustifiable to use silk for this purpose, considering the cases reported where a silk thread has formed the nucleus of a calculus.

Before the incision has been closed a bougie ought to be passed down to the bladder. This ensures that nothing has been overlooked.

On replacing the organ see that there is no kinks or twists in the ureter. I have seen a most able surgeon replace the kidney upside down. The result is disastrous, and this, together with sepsis, may be considered the chief cause of renal sinus following an operation.

I have said nothing with regard to nephrectomy. This is a most exceptional necessity, and should only be performed in rare instances where there is a large branched friable calculus accompanied by extensive sepsis, where all the secreting substance is destroyed. It should always be the aim to save the kidneys in every possible case. This is emphasized when we consider how liable these calcareous cases are to be attacked in the other kidney. These cases of nephro-lithotomy should all be drained, and I prefer to use both gauze and rubber tube for this, removing the latter at the end of 24 to 48 hours. A tight binder is always necessary, and I have found that an elastic bandage applied over this for about eight hours after the operation, is very material in combating shock. A disadvantage is that sometimes it causes a lot of pain. Morphia may be used in such a case usually without fear.

THE SURGICAL ASPECTS OF SYPHILIS.

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The surgical aspects of this disease are wide, and can be dealt with from many standpoints. Surgical treatment began at one time, at least, with the excision of the primary chancre itself. With the discovery of the spirochaete pallida by Schaudinn and Hoffman, a further interest was given to this procedure, as the presence of this organism permits of a definite diagnosis being made. The value of this as a clinical method,

however, is still confined to the hands of a few, and is by no means of universal application. Excision has been practised in very early times, has fallen into disuse, been again adopted, and again abandoned. Still excision is believed in by a few whose authority is entitled to consideration. Ricord, than whom no greater authority ever lived during his lifetime, at least is credited with the statement that "even if we amputated the penis as soon as the chancre appeared, syphilis would none the less certainly follow. (Osler, *Modern Medicine*. Edition 111.) The same writer adds: "The treatment of the chancre then consists in doing nothing." This is easily enough done, but I question if even the dictum of such an unquestionable authority should be literally adopted. I have not met with any reference to the propriety of combining removal of the inguinal glands with primary excision of the chancre itself. This would, no doubt, be rather a formidable undertaking, but none the less the disease itself is truly a terrible one, a fact only half appreciated by the laity. I have no hesitation in saying, speaking, of course, with all reservation from a limited experience, that medical treatment ought to be instituted at a far earlier period than it is. To wait until unmistakable symptoms of constitution infection have developed certainly appears to me to be a mistake. It is difficult at times to know whether the primary sore is a true Hunterian chancre or not. In such cases why not begin treatment at once? If the lesion turns out to be a simple innocuous sore, what harm is done by the treatment? If a genuine chancre develops, then the gain of the early treatment instituted can scarcely be overestimated. The surgical aspects of syphilis then, would not seem to be of much importance at this stage. This applies with equal force to the bearing of surgery on extra-genital chancres as well.

In our inquiries in connexion with a number of surgical maladies we should never forget the rôle that syphilitic infection may be playing. In an article on Syphilis in Surgical Practice (*American Year Book of Medicine and Surgery*. Gould, 1901) A. D. Bevan calls attention to the great number of cases presenting symptoms of various kinds, which will readily depend on the syphilitic infection, and suggests that this disease ought always to be borne in mind. He quotes as an example "a sore on the hand which produces axillary enlargement, which persists for more than ten days, which is locally limited, which is accompanied by a slight continuous elevation of temperature, is suspicious of syphilis, no matter what the size, shape, or consistency of the sore may be." My late personal experience was this. A young man, 30 years of age, in the best of health, asked my advice regarding a sore which he had on the very tip of the index finger of his left hand. It was less than the size of a threepenny piece, was ulcerated, but not deeply in the centre, had a somewhat hard base, and was the seat of a very small amount of discharge, hardly enough to moisten the dressing he had had on for some hours. In answer to my inquiries he said he thought that he had hurt the tip of his finger in some way, but was not sure. The affection seemed to be very small and trivial, but as he was serving in a shop he was most anxious to have it healed as it interfered with his work. I prescribed the usual remedies, and told him to keep his arm in a sling. He had noticed some redness in the part a few weeks before coming to me, but it was so trivial that he took no notice of it. To my surprise the little sore would not heal, even after some days, and trying various remedies. Then he complained of tenderness in the axilla, and on examination there was glandular enlargement,

due, as I thought, to some septic absorption. Two days after he complained of sore throat, thought he had caught a cold. Matters continued for a few days longer in this unsatisfactory state, when he developed a universal symmetrical maculo-papular eruption. The diagnosis was then no longer difficult. It is with the tertiary or gummatous manifestations of syphilis that surgery has mostly to deal, and here it is of a real and lasting service. Here again diagnosis plays a prominent part, as the resemblance of gummatous collections simulate malignant disease at times very closely. Here is a case in point. A man, aged 50, complained of a lump or growth in his gluteal region. It was half as large again as the shut fist, was fairly soft, well-defined, round in contour, and firmly fixed to the part. It was not tender on pressure, and the skin covering it was a little discoloured. It had grown rather rapidly. The patient appeared well, but had a slightly cachectic appearance. The tumor was easily removed. It was noted that it had no distinct capsule, and was somewhat infiltrating in the adjacent tissues. Recovery was quite normal, and apparently perfect. The patient returned to hospital some months after he had been discharged "cured," with a recurrence of the growth. It was of moderate size, and occupied chiefly one end of the wound. Its physical features were much the same as the previous one, only it felt softer. On exposing it, it was found to be quite diffuent, and most of it was removed with a large curette. On a careful examination by prepared sections the growth was found to be a gumma, and no doubt had been from the very first. The patient was put on mercury and iodides, and the wound soon soundly healed. Nor had it recurred years afterwards. A very striking instance of the resemblance of a gumma to sarcoma occurred in the person of a married lady, the mother of five children. A lump or growth developed on the inner aspect of the right thigh between the groin and the knee. Small at first, it was taken little notice of till it had assumed fairly large proportions. The leg began to feel stiff with a good deal of pain in it, especially at night in bed. I saw the patient when the growth was considerably larger than a good-sized egg. It was smooth, not lobulated, and very distinctly defined. It was moderately soft in consistence, had some horizontal, but no vertical mobility. A few enlarged veins coursed over it. The thigh was somewhat swollen, and in the neighbourhood of the tumour there was some pitting. She accounted for the slight inflammatory condition by stating that a few days before she had struck the part against the edge of the dresser. The patient's general appearance was none too good, she had lost flesh, was not sleeping well, and her appetite was poor. After a few days' preparation the growth was removed, and the wound healed quickly and well. The leg became its natural size, the pain disappeared, and it appeared all right. A month after a recurrence took place at the lower angle of the wound, which, when I saw it, had attained the size of a pigeon's egg. It had the same physical features as the first, and was also removed. The wound healed well. Becoming suspicious I put her on iodide of potassium and mercury, recommended a change of air, and a more liberal diet. Her general health improved well, but a small nodule again recurred close to the old site. There was some tumefaction of the leg in the neighbourhood, and a good deal of pain, especially at night. I now regarded the recurring growth as sarcomatous, microscopical findings being also in that direction. The matter now looked serious, as it became a question as to whether her leg ought to be amputated or not. She consulted Mr. F. D. Bird. He regarded the condition as gummatous, and

recommended larger doses of mercury and iodides. Also mercurial inunctions, so as to get the system as quickly as possible under its influence. This was pushed till slight salivation occurred. The dose was then diminished, and the drug continued for a long period. At the commencement of the treatment the nodule was removed. There was no recurrence, the tumefaction of the leg disappeared, and the leg itself became quite normal in shape. The pain also left, and did not return. That is now three years since, and the patient remains perfectly well. The lesson to be derived from such a case as that is too obvious to require any comment. The very fact that the question of the sacrifice of the limb was considered at all, shows how narrowly disaster was averted. Gummatous affections in the nose pharynx and larynx are far from infrequent. I have at the present moment a patient under my care with gummatous ulceration of the left nasal cavity. When first seen the nose generally was much swollen, painful, and the seat of a foul and offensive discharge. There was nothing in the history of the case to raise the least suspicion of specific disease. The disease was also involving the naso-pharyngeal space and the end of the left Eustachian tube. I was contenting myself with keeping the part clean with nasal washes, and lessening the patient's discomforts. Some weeks after first seeing him a nodule nearly as big as a small filbert developed exactly in the centre of his soft palate. This broke down, and the part became perforated. I at once had the original seat of the disease well cleansed and curetted, and put the patient on iodides and mercury. Improvement set in at once, and the disease has now almost totally disappeared, the clean, almost punched-out looking hole in his palate, being the only evidence of the disease left.

I was hurriedly asked some years since to perform trachæotomy on a middle-aged woman who was stated to have malignant disease of the larynx. I did so successfully, and had the opportunity of examining and watching the case afterwards. The ventricle and cords were extensively indurated, and thickened, and in parts ulcerated, the whole appearing to be matted together. A very small aperture only could be distinguished. Some enlarged glands could be felt in the neck. Her general health improved considerably after the tracheotomy, and she recovered sufficiently to go about her work. It occurred to me that it might be as well to try specific treatment, although I had not become aware of any facts that would lead me to think it would be of any use. It was prescribed more for appearances than anything else. The results were striking. In a few weeks she quite recovered her usual health, and felt as well as ever. I have seen her constantly since, and in every way she is a healthy woman. She still wears a tube with which she cannot dispense. Great difficulty will be experienced at times in distinguishing between gummatous disease and tuberculous disease of the testicle. Orchitis is frequently a manifestation of syphilis, though perhaps somewhat late. In the early stages there is not much difficulty in deciding, but when the size is considerable, it is often no easy matter. Tuberculous disease, as we know, first invades the head of the epididymis, but in an advanced stage this feature counts for very little. Malignant disease at this stage may also have to be reckoned with. It is unnecessary to particularize the various distinctions, except to remember that a mistake may be very easily made. The condition of the spermatic cord will very materially help. Indeed, we are confronted at nearly every step with pitfalls in dealing with this most manifold form of all diseases. Some time since I was consulted by

a jockey for an enlarged gland in the left inguinal region, about the size of a pigeon's egg, hard to the feel, freely movable, and quite painless. I could find no evidence of a sore, and concluded that it was due to some unknown trivial cause. On my suggesting some local treatment, he begged for its immediate removal, as he was going to shortly ride in a race, and in bending forward in the saddle it hurt him. He was admitted to the hospital, and two days after I removed it without, as may be supposed, any trouble. After the sutures had been inserted, and in the act of lifting up his undergarments to apply a bandage, I noticed a rash on his abdomen, which I thought peculiar. This proved to be an extensive symmetrical specific macular rash. Other evidences of secondary manifestations were at once forthcoming on inquiry. Without the least suspicion I had removed an enlarged gland secondary to an overlooked or insignificant chancre. Instances of this nature might be indefinitely multiplied, all tending to the same end. The question of the surgical aspects of visceral syphilis is extremely important, but of which I have had very little experience. Many cases of such will, no doubt, furnish very similar experiences. May I be permitted to mention one case that surely must be rather rare? I was consulted by a middle-aged man for very decided symptoms of dysphagia. This was found to be due to an obstruction close to the cardiac end of the stomach. The stenosis had progressed to such an extent that he was reduced to swallowing liquid food entirely, and even in the swallowing of that he was beginning to experience a difficulty. He had lost 4 stone in weight, and from being a hale, hearty, vigorous man was reduced to a pitiful state of misery and semi-starvation. I gave the worst possible prognosis, as certainly the circumstances warranted. I explained that his life might be prolonged for a time by operation, but this was declined. To my surprise and, you may be sure, pleasure, though great perplexity, I heard long afterwards that he was decidedly better. I have reason to believe that at the present moment he is comparatively well, though three years have elapsed. The solution of the apparent mystery was rather commonplace. It came to my knowledge that he had been treated for undeniable specific disease a few years previously. No doubt the obstruction was a gumma that had subsequently ulcerated. These few cases, culled from my own experience, while within the limits of "The Surgical Aspects of Syphilis," is more a small contribution to the oft-repeated maxim—"Bear in mind the existence of syphilis in many of the manifold manifestations of surgical diseases that we are called upon to treat."

THE TREATMENT OF SYPHILIS BY ARYLARSONATES.

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As a result of the apparent beneficial effects which had been obtained from the use of "Atoxyl" in sleeping sickness, it was suggested by Uhlenhuth that this drug might also prove successful in syphilis, the latter being like sleeping sickness, a protozoal disease. Hallopeau and Salmon made experiments according to this suggestion with a certain amount of success, and in August, 1907, I began a series of them at the Military Hospital, Rochester Row, London, with the view to determining whether there was anything in the suggestion, and began the treatment of a certain

number of cases of syphilis by Atoxyl in August, 1907. From that date until now, the treatment has been under observation. The time during which it has been carried out may be divided into three periods, *i.e.* :—

August, 1907, to 1st November, 1907.

1st November, 1907, to 1st March, 1908.

1st March, 1908, to 1st July, 1908.

During the first period, nineteen cases were treated, the preparation known as "Atoxyl" being alone used in carrying out the treatment. The drug was administered intramuscularly first of all. Three grs. were given every third day for ten days, when an interval was allowed to elapse before any more was administered, then in the dose recommended by Hallopeau, of Paris, *e.g.*, 0.75 grammes, was first given; two days later 0.65 grammes, and three days after, 0.50 grammes; but I modified his idea, in that I repeated the course in ten days' time.

All the cases so treated did well, in spite of my being handicapped in having to work more or less in the dark as regards the dose which was physiologically necessary, and at the same time safe. Although the number of cases dealt with in this first period were few, and the time during which they had been under observation short, still the results were encouraging, and treatment was continued.

During the second period, November, 1907, to March, 1908, 25 cases were treated, but during this time, for reasons to be afterwards explained, a new preparation of Arylarsonate called "Soamin," was substituted for "Atoxyl," which was entirely discarded. Of "Soamin," a total of 2,260 grains was given during this period, or an average of 90 grs. to each patient. The dose which was given at each injection being 6 grs., which was administered every third day until the total of 90 grains was obtained.

The results of the treatment during this period were embodied in a paper by Major W. A. Ward, R.A.M.C., which appeared in the *Journal of the Royal Army Medical Corps*, for April, 1908, from which it will be seen how satisfactory they were, and the following is an extract from the paper referred to :—

"In view of the experience of others who have used so-called "Atoxyl," and the symptoms of intolerance and toxicity recorded by them after the administration of comparatively small doses, it is a very interesting fact that, so far we have not had one single case in which the patient has complained of any symptoms which could be caused by the administration of this drug. On the other hand, all the cases have done remarkably well. They have gained in weight almost immediately after they have come under treatment, and when their ordinary diet had not been supplemented by "extras." This beneficial action of the drug would appear to be most marked in those cases where there is ulceration of the mouth, tongue, and throat. In five out of 25 treated, this was a prominent symptom, but a marked change in the condition was noticed even after the second or third injection, by which time the ulcer had already assumed a much cleaner condition and healthy granulations had appeared. In the course of a week, what had been a foul, dirty-looking syphilitic ulcer had entirely healed. The action of the drug was also well marked in three cases of vegetating condylomata, one of which was a most severe case."

I found that as a rule all active symptoms had disappeared after eight or ten injections. In the majority of cases, when there was a return of these, a period of from eight to ten weeks had elapsed before they made their appearance, and then they were very slight, and rapidly yielded to a second course of Soamin injection of 6 grs. each, which was the dose used in the latter part of this period. Up to 1st March, ten of the 25 cases had only had 60 grs. of Soamin, but up to that date, had had no return, and had then been free for eight months. Three others, which had received one course of 90 grs., had been quite free of the disease for seven months without any further treatment. All of these five cases had had very definite symptoms to start with, such an induration of the sores, macular-papular eruptions on the trunk and arms, alopecia, and general adenitis.

During the third period, *i.e.*, 1st March, 1908 to 1st July, 1908, 30 cases were treated solely by "Soamin." During that time. I have not seen a sign of any toxic effect of the drug, although the dose has been considerably increased. I now give grs. X. at each injection, until a total of 100 grs. have been given; the injection is administered every alternate day. All the cases have done remarkably well, of which the following are some of the most prominent:—

1. Private W., 1st Scots Guards, admitted 22nd April, 1908. Large indurated sore at root of penis, glands in groin and back of neck "shot," roseolar rash scattered over trunk, and nocturnal headaches. Was put on "Soamin" injection, receiving at first 6 grs. at each time, increased afterwards to grs. X. Up to 30th May he had received a total of 100 grs., his condition then being—gained 7 lbs. weight, condition generally improved, induration disappeared, rash also completely gone, and there was no active sign to be seen, with the exception of the enlarged gland. Further treatment was suspended. Up to date, although closely watched, has had no further developments.
2. Private H., 2nd Coldstream Guards, admitted 24th February, 1908, with what looked like a simple venereal sore at corona glands, which healed completely with local treatment: eventually it became indurated. On 3rd April was re-admitted to hospital with the following symptoms:—Indurated lump at site of original sore, shot inguinal and cervical glands, and a fairly profuse roseolar rash on back and on the chest, throat slightly ulcerated. Soamin injections of grs. X were given every alternate day until the total of 100 grs. was received, when he was discharged from hospital, all the active syphilitic symptoms, including the induration, having disappeared; he had gained 6 lbs. weight.
3. Corporal A., 2nd Grenadier Guards, admitted on 3rd April, 1908, with induration at the scar of a circumcision, inguinal, cervical, and mastoid glands bullet-like. Received injections of Soamin regularly every alternate day in grs. X doses until 100 grs. were given. Was discharged from hospital on 27th April, 1908, his then condition being:—much improved in health, induration disappeared, and had gained 10 lbs. weight whilst in hospital. No further treatment, closely observed, but up to date, 15th July, 1908, remains free from further signs.

4. Private T., 2nd Grenadier Guards, admitted 22nd April, 1908, with hard phagedænic sore on foreskin, glands in groin, behind neck and mastoid enlarged and hard, profuse macular-papular eruption on chest and abdomen, and severe ulcerated throat; was evidently a neglected case. Put on X grs. Soamin injections every second day until he had had 100 grs. Discharged from hospital 27th May, 1908, when the sore had completely healed, and induration quite absorbed, general health much improved, and he had gained 8 lbs. in body weight. The rapid manner in which the sore healed under constitutional treatment alone was remarkable in this case. It was thought advisable that the patient should receive a second course of treatment, and since his discharge, he has had 50 grs. of Soamin, and has had no further development.
5. Private H., 1st Grenadier Guards, admitted 27th April, 1908, with the following symptoms:—Hard parchment-like sore on foreskin, deep inguinal, cervical, and mastoid glands enlarged, roseolar rash on legs, arms, and chest, throat badly ulcerated. Put on Soamin grs. X injections every second day. Received 100 grs. in all, and was discharged from hospital on 19th May, 1908, all active signs having disappeared; he had gained 7 lbs. in weight.
6. Private C., 1st Grenadier Guards, admitted 21st May, 1908, with remains of induration at scar of circumcision, inguinal and cervical glands bullet-like, sore throat, ulcers on tongue and macular-papular eruption on body and legs. Treated solely by injections of Soamin, of which he received a total of 100 grs. All active symptoms rapidly disappeared, especially the ulcerated throat and tongue, without any local treatment; general health much improved, had gained 8 lbs. weight. Left hospital on 10th June, 1908.
7. Private H., 1st Scots Guards, discharged from the Royal Herbert Hospital, Woolwich, 15th April, 1908, where he had been under treatment for a "soft chancre" for two weeks. Re-admitted at Rochester Row, 27th April, 1908, and denied having been in the way of re-infection since his discharge from Woolwich. On admission at Rochester Row, had the following symptoms:—old sore near corona glands had broken down, and was surrounded with well-marked induration. No other signs, owing to typical character of sore. Was put on Soamin, and received the full course of 100 grs. Discharged from hospital on 25th May, having gained 4 lbs. in weight, sore healed, and induration disappeared. This man has had no further treatment, but has been under close observation, up to date, 10th July, has had no further developments of the disease.
8. Private C., 2nd Grenadier Guards, discharged from the Royal Herbert Hospital, 25th May, where he had been under treatment by inunction for a venereal sore on glands penis, which was described as "slightly indurated." Re-admitted to Rochester Row 22nd June, no induration at site of sore, adenitis of deep inguinal and cervical glands, roseolar rash

on body, ulcerated tongue and throat, and general debility. Received full course of Soamin, and made rapid recovery. Was discharged from hospital, 20th June, all active signs having disappeared.

9. Lance-Corporal V., 2nd Grenadier Guards, admitted, 30th April, 1908, with a well-marked indurated chancre at base of prepuce coming on four weeks after exposure. Spirochæte palada demonstrated in it. Put on Soamin injections, receiving a full course of 100 grs. Induration rapidly gave way, and he left hospital free from all symptoms on 28th May. No further treatment and no further development of disease up to date of last inspection, 10th July, 1908.
10. Private V., 2nd Grenadier Guards, admitted 10th May, 1908. Indurated sore on glands penis, Spirochæte P. found in a "smear" from it. Inguinal, cervical, and mastoid glands shotty; profuse macular-popular eruption scattered over body and legs. Ulcerated throat, debility and general anæmia. Received usual course of 100 grs. of Soamin by injection. When he left hospital on 25th June, all active signs had cleared up; he had improved generally in health, having gained 8 lbs. in weight.
11. Corporal G., 1st Scots Guards, admitted 10th May, 1908, with a well-marked parchment-like induration on prepuce at site of an old sore, for which he had no specific treatment, no glandular enlargements or other signs. Spirochæte P. demonstrated in fluid drawn from induration. Put on Soamin at once, of which he received 100 grs. He left hospital apparently free of all symptoms on 20th June, and has had no further return of disease.
12. Private J., 2nd Grenadier Guards, admitted 3rd June, 1908, with multiple sores on glands penis and prepuce, two of which were distinctly indurated, and in which the Spirochæte P. were demonstrated, adenitis of inguinal and cervical glands, profuse roseolar rash on abdomen and legs, ulcerated throat and severe nocturnal pains. Had 100 grs. of Soamin. All symptoms cleared away.
13. Corporal H., 1st Scots Guards, discharged from the Royal Herbert Hospital, Woolwich, on 4th May, 1908, having been under treatment there for a venereal sore, but had had no specific treatment. Re-admitted at Rochester Row on 11th June, 1908, with induration at site of old sore, roseolar rash scattered over chest and abdomen, bullet glands in inguinal and cervical regions, ulcerated throat, and anæmia. Under Soamin injections, all the above signs rapidly disappeared, and he was discharged from hospital 29th June.

These are the notes of a few of the cases treated during the third period, the remainder are very similar. Although it may be urged that the number of cases so treated are too few, and the time which has elapsed since the treatment ceased too short to allow of a definite opinion being come to concerning it; still, so marked is the success one has seen during the past eleven months, that I certainly feel justified in expressing myself as fairly convinced on the following points. I consider that the prophylactic effects of the drug are very marked, and that when administered

early, and in full doses, it certainly seems to check any further developments, *vide* cases above quoted, Nos. 3, 7, 9, 11. I have every reason to believe that these cases were genuine primary syphilitic chancres which disappeared under the treatment, and, up to date, show no further signs of the disease. I have no doubt that this treatment modifies distinctly the secondary signs of the disease which, if they do appear, are generally of a peculiarly mild type, and readily yield to further treatment, besides which they are delayed for a long time in making their appearance.

It has a most beneficial effect in any existing secondary or other active symptoms, causing them to disappear quickly, especially is this the case with mucous patches in the mouth and ulceration of the throat. With reference to the latter, it has been very noticeable that since the introduction of the treatment by these salts at Rochester Row ulcerated throats have become very few, and at the present moment it would be hard to find one. This is an astonishing fact in a special venereal hospital.

Under this treatment patients begin to gain weight almost at once, and their anæmic condition and general health to improve, and one cannot help being struck with the general aspect of patients undergoing it.

So far, I have seen no signs of toxicity or other bad effects from this drug.

What are Arylarsonates Salts.—The term "Arylarsonates" indicates those arsonates to which an aryl group is attached. The arsonic acids may be considered to be derived from arsenic acid by the replacing of the hydroxyl group by an organic radicle. The organic radicle may be a member either of the fatty or paraffin series, such as methyl, ethyl, &c., or of the aromatic or benzene series, as phenyl. The radicles of the former are known as "alphyl," and an arsonic acid of this type would be known as "alphylarsonic" acid. When the radicle belongs to the aromatic or benzene series, it is called an "aryl" group, and the arsonic acid would be an "arylarsonic acid." In the case of "Atoxyl" and "Soamin," the aryl radicle is aniline. Briefly, the prefix "aryl" to arsonates, indicates a radicle belonging to the aromatic series.

Preparation used.—As already stated, the treatment was begun and continued through the first period with the preparation known as "Atoxyl," which was the original arylarsonate preparation used; it was introduced by a German firm, and was said to be a meta-arsenicanilide, having the formula $C_6H_4(NH_2)(AsO.OH.ONa)(4H_2O)$, and containing 37.69 per cent. of arsenium. Analysis showed, however, that it contains only 25.77 per cent. Further investigations showed that Atoxyl is really a sodium salt of arsenic acid, in which one hydroxyl radicle of arsenic acid has been replaced by the aniline radicle. Agreeing with this, the manufacturers adopted the formula $CH(NH_2)(AsO.OH.(ONa)4H_2O)$, as indicating the nature of the substance. The Codex of the B.P. gives a synonym for atoxyl sodium anylarsonate, and the formula $C_6H_7NAsO_3 \cdot Na \cdot 2H_2O$, and contains 27.3 per cent. of arsenium.

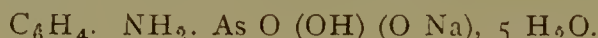
When first introduced, this preparation caused little or no trouble in the way of toxicity, but with further demands for it, apparently adulterated, or badly-made preparations, were put on the market; whatever was the cause, many cases of toxic poisoning from it were reported. Professor Hallopeau recorded symptoms of intolerance after a varying number of injections. These consisted of gastro-intestinal pains, malaise, nausea, vomiting, painful micturition, and painful sensations in the limbs. From other parts came records of even more serious results, which ended in total blindness; among others, Professor Koch reported several cases of the latter

which had happened among his patients in Central Africa. In Uganda, I found that the medical men in charge of the Sleeping Sickness Camps, were more than nervous about giving the Atoxyl recently supplied, owing to the unfortunate results they had observed following its use; and whilst there, I saw three cases of total blindness the result of it.

At the same time, as stated before, although for the first period, August, 1907, to end of October, 1907, in which the treatment was carried out at Rochester Row, Atoxyl alone was used, I saw none of these bad effects, the probability being that the preparation I was using was some which had been supplied before the run on the drug had taken place, and was consequently pure. However, with all the reports we got as to the toxicity following its use, one had to look on the drug with suspicion, and since then up to now, the treatment has been carried out by means of an arylarsonate preparation which is manufactured by Messrs. Burroughs and Wellcome, of London.

This they have named "Soamin," and describe it as follows:—

It is Sodium para-aminophenylarsonate having the following formula:—



It is this latter which has been employed by me since 1st November, up to date. The manufacturers call it "Soamin" for short. It contains 22.8 per cent. of arsenic, equivalent to 30.1 per cent. of arsenious acid. It is soluble in five parts of cold water, giving a bright neutral solution, which can be sterilized. It is incompatible with acids and solutions of heavy metals, or of quinine. Its toxicity is less than 1/40th that of arsenious acid.

For this preparation, they claim that it is a pure para-aminophenylarsonate, free from arsonates and arsonites, possessing all the properties of arylarsonates, and being free from any toxic dangers.

Since 1st November, 1907, as already stated, I have used this preparation in lieu of atoxyl, and have been able to watch its results very carefully, and I have not seen one case showing any signs of toxicity from it; the results of the cases have been most successful, and, from a therapeutic point of view, I have no hesitation in saying that it is preferable to atoxyl. It appears to have more rapid effects on syphilis than the latter, and there is no reason to think otherwise than that it is a safe preparation.

Dose.—As clinical experience could alone enable one to form any opinion as to what might be considered a correct dose of this preparation, both from a physiological as well as from the point of view of safety, I was obliged to proceed cautiously, and hence slowly, in order to feel my way. Beginning with doses of grains 3 every third day, I gradually raised it to what I have at present stopped at, *i.e.*, grains X every other day, until a total of grains 100 has been given; as I have obtained all the necessary physiological effects I desired with this dose, I do not think it wise or necessary to push it any higher, as it would subject patients to uncalled-for risks. At the same time, I have little doubt that, with further knowledge of the action of these salts, they will be administered in much larger doses with benefit.

Mode of Administration.—The arylarsonate salts should not be given by the mouth, as they are broken up by the acid contents of the stomach, and the effects of over-treatment by arsenic are thus more easily produced.

It is advisable that the solution used should be prepared freshly before use, as if kept it is apt to become decomposed.

It is given by intramuscular or subcutaneous injection, and I generally utilise the former, selecting the upper third of the buttock for site of injection. The usual steps to insure strict antisepsis must be taken, using boiling oil, or the latter raised to at least 160, to sterilize the syringe and needle; under no circumstances should an acid be used for this purpose, as acids decompose these salts.

Before injecting the solution it should be heated to body heat.

The arylarsonates should not be used simultaneously for treatment with mercury, as the latter dissociates the ingredients of any of the preparations of the former, and may give rise to accidents. Before beginning mercurial treatment, or *vice versâ*, it is well to wait for fifteen days until either is eliminated from the system.

Questions which still have to be decided are—

How we should proceed after the patient has had one full course of soamin and all active signs of syphilis have disappeared? When to begin a second? When is it safe to discontinue treatment altogether? Time and more extended clinical experience can alone decide these questions.

Of the 65 cases treated by arylarsonate at Rochester Row, ten which had had no recurrence of symptoms had a second course, and, so far, have shown no further signs of the disease. The remaining 55 were discharged from hospital when they had completed one course, and to them no further treatment has been given, with the exception of the cases which were re-admitted; the following are the results:—

Forty-nine cases, although kept closely under observation, have had no further development of the disease.

Eight men were re-admitted for further recurrence, two of them for breaking down of the old sore, two for re-appearance of rashes, two for mucous patches and ulcers of throat, one for chancre of lip broken down, and one for pustular eruption. It was noticed that these recurrences were very mild, and disappeared after a very short course of soamin.

CONCLUSION.

It may be hazardous on my part to form any definite opinion as to the benefits, &c., to be derived from the treatment of syphilis by the arylarsonates after only twelve months' experience of it; however, I feel justified in saying—

1. That there is strong reason to believe that, given early and in sufficient quantities, they can be looked on as prophylactic in the majority of cases against any further development of the disease.
2. That they undoubtedly delay and modify very remarkably the secondary symptoms.
3. That they appear to exert a marked beneficial effect on all syphilitic ulcerations.
4. Finally, without entering into the question as to whether the treatment of syphilis by arylarsonates is as efficacious, or likely to become more so, than that by mercury, which time and further experience can alone tell us, I consider that one important fact has been established, *i.e.*, that in these salts we are now in possession of a second specific for syphilis,

and need not add that the importance of this cannot be well exaggerated. With regard to the therapeutic action of arylarsonates on syphilitic lesions, the work of several observers has indicated that they do not kill the germ (as mercury is supposed to do), but exert their influence in strengthening the phagocytic defence of the host.

[NOTE.—Colonel Lambkin has not had an opportunity of revising this paper. Those interested should communicate with him directly. It is correct according to the M.S.—ED.]

SYPHILIS AND CANCER.

M. CRIVELLI, M.D., PARIS.

It is generally admitted that when constitutional diseases are met with in the same person they do not remain independent, but exert an influence upon one another, which is beyond all doubt.

The co-existence of several neoplasms, especially of benign ones, is a fact which is frequently observed.

When a malignant tumour has rooted itself in the system, it is a frequent occurrence to see it progressing on the spot or at a distance, and giving, in a variable time, a series of tumours constructed on the model of the parent one.

This is a phenomenon of generalization that has been observed by every medical man, and no longer needs describing.

These tumours, although they do not always generalize themselves, often relapse on the same spot, more tenacious after each excision, and result in discouraging even the most daring and venturesome surgeon.

The fact of so many benign and malignant tumours cohabiting in the same subject, developing on the same ground, thriving side by side, cannot be the result of mere chance. We know that, not only both benign and malignant tumours can co-exist and still be independent of one another, but also that they can follow each other, or combine together.

The most benign tumour, whose ordinary evolution is not attended by a general spreading, may suddenly change its habit, and become of the most dangerous character.

The rodent ulcer of the face frequently follows an insignificant wart; the epithelioma of the lips is often the transformation of a simple labial papilloma; and, lastly, has not the terrible cancer of the tongue often had as origin a simple hypertrophic development of the papillæ of the tongue?

Its first period is benign, but is quickly followed by the evolution, in an inverted way, of the epithelial cells, which, instead of spreading on the surface of the papillæ, dip into the thickness of the interpapillary furrows, and so penetrate into the muscular interstices of the tongue. Again, have we not seen the adenoma of the breast degenerating into a cancer twenty years after its first appearance?

Startling evidence of the relationship in the neoplasm is found in the complex tumours, in which the most dissimilar tissues are found collected. In a tumour of the parotid, it is frequent to find chondroma, adenoma, myoma, and sarcoma united together.

If we admit that all these pathological tissues, whether benign or malignant, influence each other during their evolution, it is logical to also believe that the tissues formed by cancerous lesions and syphilitic scars can also co-exist in the same subject and modify each other's development.

This important question of the possible influence of syphilis on cancer is far from being definitely settled. It is scarcely 40 years old. Born in 1873, at Paris, as the result of a few observations published by Ricord, it has been taken up again, amongst others, by Fournier and Hutchison, and as recently as a year or two ago by Ozenne, Ricard, and Lerulle, and especially since the discovery by Schaudinn of the spirochete or *treponema pallidum* has given to the diagnosis of the syphilis a character of scientific certainty it had never had before.

Three possibilities may occur: Either the syphilis is recent or ancient when the cancer develops, or the cancer is already present when the syphilis appears.

When the syphilis is quite recent, the cancer is very often the cause of a recurrence of the specific manifestations, which appear nearly always on the spot already invaded by the cancer; or, conversely, the cancer may reveal itself, in the shape of multiple foci, there where the syphilis has raged the longest and with the greatest intensity; that is to say, on those spots that have become "*loci minoris resistentiæ*."

If the venereal infection has dated from a far-back period, and has been properly treated, and a cancer subsequently develops, it is possible, if nothing unforeseen happens, that this cancer will show itself in its ordinary way, and evolve as if the ground had never been syphilitic, for all cases of cancer are not necessarily influenced by syphilis. Finally, if a syphilitic chancre is contracted during the evolution of a cancer, this chancre always stimulates the cancer wherever it may be situated in the organism.

As far as the localization is concerned, the most common situation of cancerous syphilitic lesions is the one constituted by the deep induration of the bucco-pharyngeal mucous membrane. This lesion shows itself on the tongue, the lips, or the tonsils, and its appearance varies according to the smaller or greater share of the cancer or the syphilis.

When one considers the large area of bucco-pharyngeal mucous membrane, and the diversity of the regions it covers, and also the predilection shown for it by syphilis, with its triponema and toxins, it obviously represents a choice ground for the study of such lesions.

The very numerous inflammatory troubles that are observed on it, even the most common, the alimentary ones, for instance, become a pathogenic danger by their frequent re-appearance.

Likewise, the constant irritability maintained by the habit of alcohol and tobacco, particularly on the surface of the tongue, at the commissures of the lips, at the internal surface of the cheeks, must also be kept in view. When on this membrane erosions, fissures, local infections are formed, the parasitism of the mouth cavity and the influence, often noxious, of the hydrargyric treatment constitute an abundance of proof towards admitting that the cancer of the mouth is often very closely connected with the syphilis.

Fournier goes so far as to say that in the great majority of cases the cancer of the tongue is nothing but a late complication of the syphilis.

Further, some very special manifestations, most frequently specific, described under the name of lingual psoriasis, justify the predominant

place held by the tongue, cheeks, and lips in the controversy for or against the part played by syphilis in the genesis of cancer which has taken place amongst scientific men.

In the very frequent case of the co-existence of a recently developed cancer of the buccal mucous membrane with some clearly syphilitic lesion, the cancer and the syphilis behave towards each other as we have already indicated, according to the recent or ancient character of the syphilis. These lesions are seldom observed quite at the start, for the functional symptoms are very unpronounced.

It frequently happens that the lesions have been in existence for a considerable time without having given rise to any anxiety. Either the patient thinks his ailment the consequence of the previous syphilis, and has recourse to the specific treatment, or he considers as insignificant the crack, the fissure, or the wart that rests on an indurated base. He only begins to get anxious on the day when the thickened tongue has a notable difficulty in accomplishing its motions, namely, when mastication, phonation, and deglutition become embarrassed.

The age of the subject, the seat of the affection, the irritating treatments applied at the beginning, influence the progress of this double process. But if the disease is left to itself, a day comes when the syphilitic manifestations are, so to say, smothered in and absorbed by the cancerous process which takes the field and becomes absolutely master of the situation.

All this, of course, applies equally well to the development of syphilis and cancer of the breast, the testicle, the bladder, the liver, or the rectum, as to that of the mouth.

Unfortunately, the prognosis is always associated with that of the neoplasm itself. If it is sometimes possible to obtain a temporary improvement by the iodine treatment in this dual morbid process, the last word always remains with the cancer.

When a cancer appears in a subject who has already contracted syphilis, or where there is only a suspicion of it, the medical man must advise the patient to submit himself for a little while to the specific treatment. Nevertheless, a certain restriction must be kept, a limit which ought not to be passed. The treatment must not be kept on too long, and, as soon as its uselessness is noticed, it is wise to suppress it, for its prolonged use would only increase the evolution of the cancer, and, in helping its extension, would make more uncertain the results of the surgical intervention.

In a cancer complicated by syphilis, surgical intervention is not contra-indicated, and it can be operated in the usual way, for there is nothing to prevent the use of specific treatment after the operation.

All new cases of cancer in a damaged constitution, in a ground already disorganized by colonies of specific "treponema," deserve to be related and closely studied, for they will constitute a startling argument in favour of the scientific doctrine, the pathogenic axiom that makes syphilis one of the great predisposing causes of cancer.

A SUGGESTED TREATMENT OF EXTENSIVE SUPERFICIAL BURNS WITH THE VIEW TO AVOID SUBSEQUENT SCARRING.

HON. J. M. CREED, M.R.C.S., L.R.C.P.

In the case I present to the Congress, the treatment proved so marked a success that I feel it is not unfitting that I should state the method I adopted, which, if not absolutely novel, I submit, is so unusually followed as to make the relation a matter of interest to practitioners.

A young lady was using a spirit lamp for toilet purposes, which shortly after being lighted exploded, covering the front of her head, her face, neck, and chest with the flaming alcohol. The result was that the front of her hair, her eyebrows, and lashes were destroyed, and the other parts mentioned were so burnt as to entirely destroy every particle of cuticle.

This happened on a Sunday evening, and though usually a patient of my own, my absence necessitated application to another medical man.

He followed the usual course, and dressed it with the well-known Carron Oil.

She came under my care on the following day, but I did not interfere with the original dressings until on the Wednesday I found a profuse purulent discharge with a foetid odour escaping from them.

I reasoned that with the usual repeated dressing of such an extensive surface, healing would be inevitably accompanied by numerous cicatrices, which on the face would be an irreparable disfigurement.

After consultation with Dr. Maitland, I therefore determined to follow a course which I hoped might avoid any necessity for more than the first dressing, without requiring further exposure of the wound surface.

After making my preparations, the oil and limewater dressings were removed, and it was found that the whole of the parts burnt was a raw bleeding surface, with free suppuration. It was bathed with a solution of mercury and potassium iodide to make it aseptic, swabbed fairly dry with antiseptic gauze, and covered by means of a flour-dredger with finely-powdered boric acid.

I directed the nurse to sit by the patient and dredge on the boric acid as frequently as the surface showed any dampness. By the second day, and always afterwards, it remained dry, and then the powder was only applied three times in the 24 hours. Nothing more was done, and in about three weeks the whole separated as a mask, leaving the surface without a scar, except a narrow cicatrix of about three-quarters of an inch on the edge of the right lower jaw, where she had scratched an itching spot.

For some little time there was red discolouration on her getting heated, but this entirely disappeared, and now, with the exception of the scar mentioned, there is no trace of such a very grave injury.

Should I have in future to treat a burn or scald of a like character, I should apply a 1 per cent. solution of picric acid until the pain has

ceased, then remove all blisters and other loosened cuticle, make the exposed surface aseptic, and at once dredge with finely-powdered boric acid.

I should not use the picric solution any longer than during the time necessary for the pain to subside. I take this precaution to avoid the remote contingency of discolouration from its use.

I think that the chief cause of my success in preventing disfigurement was the avoidance of repeated removal of dressings. However carefully this is done, there is unavoidable damage to the tender epidermis newly formed, and I submit it is this which gives rise to the scars. The absence of the pain caused by repeated dressings is also a distinct advantage.

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